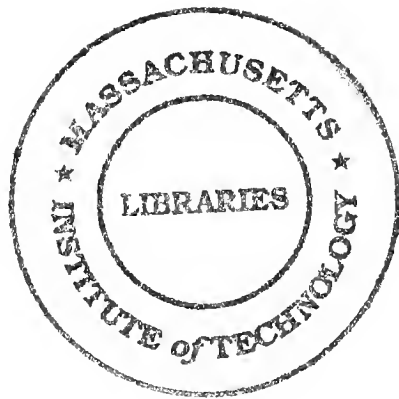


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**THE SLOWDOWN OF THE ECONOMICS  
PUBLISHING PROCESS**

Glenn Ellison, MIT Dept of Economics

Working Paper 00-12  
July 2000

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# The Slowdown of the Economics Publishing Process

Glenn Ellison<sup>1</sup>

Massachusetts Institute of Technology and NBER

June 2000

<sup>1</sup>I would like to thank the National Science Foundation (SBR-9818534), the Sloan Foundation, the Center for Advanced Study in the Behavioral Sciences, and the Paul E. Gray UROP Fund for their support. This paper would not have been possible without the help of a great many people. I am very grateful for the efforts that a number of journals made to supply me with data. In addition, many of the ideas in this paper were developed in the course of a series of conversations with other economists. I would especially like to thank Orley Ashenfelter, Susan Athey, Robert Barro, Gary Becker, John Cochrane, Olivier Blanchard, Judy Chevalier, Ken Corts, Bryan Ellickson, Sara Fisher Ellison, Frank Fisher, Drew Fudenberg, Joshua Gans, Edward Glaeser, Daniel Hamermesh, Lars Hansen, Harriet Hoffman, Jim Hosek, Alan Krueger, Paula Larich, Vicky Longawa, Robert Lucas, Wally Mullin, Paul Samuelson, Ilya Segal, Karl Shell, Andrei Shleifer and Kathy Simkanich without implicating them for any of the views discussed herein. Richard Crump, Simona Jelescu, Christine Kiang, Nada Mora and Caroline Smith provided valuable research assistance.



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## Abstract

Over the last three decades there has been a dramatic increase in the length of time necessary to publish a paper in a top economics journal. This paper documents the slowdown and notes that a substantial part is due to an increasing tendency of journals to require that papers be extensively revised prior to acceptance. A variety of potential explanations for the slowdown are considered: simple cost and benefit arguments; a democratization of the publishing process; increases in the complexity of papers; the growth of the profession; and an evolution of preferences for different aspects of paper quality. Various time series are examined for evidence that the economics profession has changed along these dimensions. Paper-level data on review times is used to assess connections between underlying changes in the profession and changes in the review process. It is difficult to attribute much of the slowdown to observable changes in the economics profession. Evolving social norms may play a role.

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# 1 Introduction

Thirty or forty years ago papers in the top economics journals were typically accepted within six to nine months of their submission. Today it is much more common for journals to ask that papers be extensively revised, and on average the cycle of reviews and revisions consumes about two years. The change in the publication process affects the economics profession in a number of ways — it affects the timeliness of journals, the readability and completeness of papers, the evaluation of junior faculty, etc. Probably most importantly, the review process is the major determinant of how economists divide their time between working on new projects, revising old papers and reviewing the work of others. It thus has a substantial impact both on the aggregate productivity of the profession and on how enjoyable it is to be an economist.

This paper has two main goals: to document how the economics publishing process has changed; and to improve understanding of why it has changed. On the first question I find that the slowdown is widespread. It has affected most general interest and field journals. Part of the slowdown is due to slower refereeing and editing, but the largest portion reflects a tendency of journals to require more and larger revisions. My main observation on the second question is that it is hard to attribute most of the slowdown to observable changes in the profession. I view a large part of the change as due to a shift in arbitrary social norms.

While the review process at economics journals has lengthened dramatically, the change has occurred gradually. Perhaps as a result it does not seem to have been widely recognized (even by journal editors). In Section 2 I provide a detailed description of how review times have grown and where in the process the changes are occurring. What may be most striking to young economists is to see that in the early 1970's most papers got through the entire process of reviews and revisions in well under a year. In earlier years, in fact, almost all initial submissions were either accepted or rejected — the noncommittal “revise-and-resubmit” option was used only in a few exceptional cases.

In the course of conversations with journal editors and other economists many potential explanations for the slowdown have been suggested to me. I analyze four sets of explanations in Sections 3 through 6. Each of these sections has roughly the same outline. First, I describe a set of related explanations, e.g. ‘A common impression is that over the last 30 years change X has occurred in the profession. For the following reasons this would be expected to lead to a more drawn out review process . . .’ Then, I use whatever time series

evidence I can to examine whether change  $X$  has actually occurred and to get some idea of the magnitude of the change. Finally, I look cross-sectionally at how review times vary from paper to paper for evidence of the hypothesized connections between  $X$  and review times. In these tests, I exploit a dataset which contains review times, paper characteristics and author characteristics for over 5000 papers. The data include at least some papers from all of the top general interest journals and contain nearly all post-1970 papers at some of the journals.

Section 3 is concerned with the most direct arguments — arguments that the extent to which papers are revised has gone up because the cost of revising papers has gone down and the social benefit of revising papers has gone up. Specifically, one would imagine that the costs of revisions have gone down because of improvements in computer software and that the benefits of revisions have gone up because the information dissemination role of journals has become less important. Most of my evidence on this explanation is anecdotal. I view the explanation as hard to support, with perhaps the most important piece of evidence being that the slowdown does not seem to have been intentional.

In the explanations discussed in Section 4, the exogenous change is the “democratization” of the publishing process, i.e. a shift from an “old boys network” to a more merit-based system. This might lengthen review times for a number of reasons: papers need to be read more carefully; mean review times go up as privileged authors lose their privileges; etc. Here I can be more quantitative and find that there is little or no support for the potential explanations in the data. Time series data on the author-level and school-level concentration of publication suggest that there has not been a significant democratization over the last thirty years. I find no evidence of prestige benefits or other predicted effects in the cross-sectional data.

In Section 5 the exogenous change is an increase in the complexity of economics papers. This might lengthen review times for a number of reasons: referees and editors will find papers harder to read; authors will have a harder time mastering their own work; authors will be less able to get advice from colleagues prior to submission, etc. I do find that papers have grown substantially longer over time and that longer papers take longer in the review process.<sup>1</sup> Beyond this moderate effect, however, I find complexity-based explanations hard to support. If papers were more complex relative to economists’ understanding I would expect that economists to have become more specialized. Looking at the publication records of economists with multiple papers in top journals, I do not see a trend toward increased

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<sup>1</sup>Laband and Wells (1998) discuss changes in page lengths over a longer time horizon.

specialization. In the cross-section I also find little evidence of the hypothesized links between complexity and delays. For example, papers do not get through the process more quickly when they are assigned to an editor with more expertise.

In Section 6 the growth in the economics profession is the exogenous change. There are two main channels through which growth might slow the review process at top journals: it may increase the workload of editors and it may increase competition for the limited number of slots in top journals. Explanations based on increased editorial workloads are hard to support — at many top economics journals there has not been a substantial increase in submissions for a long time. While the growth in the economics profession since 1970 has been moderate (Siegfried, 1998), the competition story is more compelling. Journal citation data indicates that the best general interest journals are gaining stature relative to other journals. Some top journals are also publishing many fewer papers. Hence, there probably has been a substantial increase in competition for space in the top journals. Looking at a panel of journals, I find some evidence that journals tend to slow down more as they move up in the journal hierarchy. This effect may account for about three months of the observed slowdown at the top journals.

My main conclusion from Sections 3 through 6, however, is that it is hard to attribute most of the slowdown to observable changes in the profession. The lengthening of papers seems to be part of the explanation. An increase in the relative standing of the top journals is probably another. Journals may have less of a sense of urgency now because of the wider dissemination of working papers. Looking at all the data, however, my strongest impression is that the economics profession today looks sufficiently like the economics profession in 1970 to make it hard to argue that the review process must be so different. Instead, I hypothesize that much of the change may reflect a shift in the social norms that dictate what papers *should* look like and how they *should* be reviewed.

The argument described above gives social norms a privileged status in that the case for it made by showing that there is a lack of evidence for other explanations.<sup>2</sup> It also provides an incomplete answer to the question of why the review process has lengthened, because it does not tell us why social norms have shifted. Ellison (2000) provides one potential explanation for why social norms might shift in the direction of emphasizing revisions.<sup>3</sup>

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<sup>2</sup>In some ways this can be thought of as similar to the way in which papers without any data on technologies have attributed changes in the wage structure to “skill-biased technological change,” and the way in which unexplained differences in male-female or black-white wages are sometimes attributed to discrimination.

<sup>3</sup>The model also attempts to provide a parsimonious explanation for other observed changes in papers, such as the tendency to be longer, have a longer introduction and more references.



Papers are modeled as differing along two quality dimensions,  $q$  and  $r$ . The  $q$  dimension is interpreted as representing the clarity and importance of the paper's main contribution and  $r$ -quality is interpreted as reflecting the other dimensions of quality that are often the focus of revisions, e.g. exposition, extensions and robustness checks.<sup>4</sup> The relative weight that the profession places on  $q$  and  $r$  is an arbitrary social norm. Economists learn about the social norm over time from their experiences as authors and referees. Whenever referees try to hold authors to an unreasonably high standard the model predicts that social norms will evolve in the direction of placing more emphasis on  $r$ . A long gradual evolution in this direction can be generated by assuming that economists have a slight bias (that they do not recognize) that makes them think that their own work is better than it is. Section 7 reviews this model and examines a couple of its implications empirically.

There is a substantial literature on economics publishing. I draw on and update its findings at several points.<sup>5</sup> Four papers that I am aware of have previously discussed submit-accept times: Coe and Weinstock (1967), Yohe (1980), Laband et al (1990) and Trivedi (1993). All of these papers after the first make some note of increasing delays: Yohe notes that the lags in his data are longer than those reported by Coe and Weinstock; Laband et al examine papers published in *REStat* between 1976 and 1980 and find evidence of a slowdown within this sample; Trivedi examines lags for econometrics papers published in seven journals between 1986 and 1990 and notes both that there is a trend within his data and that lags are longer in his data than in Yohe's. Laband et al (1990) also examine some of the determinants of review times in a cross-section regression.

## 2 The slowdown

In this section I present some data to expand on the main observation of the paper — that there has been a gradual but dramatic increase in the amount of time between the submission of papers and their eventual acceptance at top economics journals . A large portion of this slowdown appears to be attributable to a tendency of journals to require more (and larger) revisions.

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<sup>4</sup>Another interpretation is that  $q$  could reflect the authors contributions and  $r$  the quality of the improvements that are suggested by the referees.

<sup>5</sup>I make particular use of data reported in Laband and Piette (1994b), Siegfried (1994), and Yohe (1980). Hudson (1996), Laband and Wells (1998) and Siegfried (1994) provide related discussions of long-run trends in the profession. See Colander (1989) and Gans (2000) for overviews of the literature on economics publishing.

## 2.1 Increases in submit-accept times

Figure 1 graphs the mean length of time between the dates when articles were initially submitted to several journals and the dates when they were finally accepted (including time authors spent making required revisions) for papers published between 1970 and 1999.<sup>6</sup>

The data cover six general interest journals: *American Economic Review* (*AER*), *Econometrica*, *Journal of Political Economy* (*JPE*), *Quarterly Journal of Economics* (*QJE*), *Review of Economic Studies* (*REStud*), and the *Review of Economics and Statistics* (*REStat*). The first five of these are among the six most widely cited journals today (on a per article basis) and I take them to be the most prestigious economics journals.<sup>7</sup> I include the sixth because it was comparably prominent in the early part of the period.

While most of the year-to-year changes are fairly small, the magnitude of the increase when aggregated up over the thirty-year period is startling. At *Econometrica* and the *Review of Economic Studies* we see review times lengthening from 6-12 months in the early seventies to 24-30 months in the late nineties. My data on the *AER* and *JPE* do not go back nearly as far, but I can still see submit-accept times more than double (since 1979 at the *JPE* and since 1986 at the *AER*). The *AER* data include three outliers. From 1982 to 1984 Robert Clower ran the journal in a manner that must have been substantially different from the process before or since; I do not regard these years as part of the trend to be explained.<sup>8</sup> The *QJE* is the one exception to the trend. Its review times followed a

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<sup>6</sup>The data for *Econometrica* do not include the time between the receipt of the final revision of a paper and its final acceptance. The same is true of the data on the *Review of Economic Studies* for 1970-1974. Where possible, I include only papers published as articles and not shorter papers, notes, comments, replies, errata, etc. The *AER* and *JPE* series are taken from annual reports, and presumably include all papers. For 1993 - 1997 I also have paper-level data for these journals and can estimate that in those the mean submit-accept times given in the *AER* and *JPE* annual reports are 2.2 and 0.6 months shorter than the figures I would have computed from the paper-level data. The *AER* data do not include the *Papers and Proceedings* issues. The means for other journals were tabulated from data at the level of the individual papers. For many of the journal-years tables of contents and papers were inspected individually to determine the article-nonarticle distinction. In other years, rules of thumb involving page lengths and title keywords were used.

<sup>7</sup>The ratio of total citations in 1998 to publications in 1998 for the five journals are: *Econometrica* 185; *JPE* 159; *QJE* 99; *REStud* 65; and *AER* 56. The *AER* is hurt in this measure by the inclusion of the papers in the *Papers and Proceedings* issue. Without them, the *AER*'s citation ratio would probably be approximately equal to the *QJE*'s. The one widely cited journal I omit is the *Journal of Economic Literature* (which has a citation ratio of 67) because of the different nature of its articles.

<sup>8</sup>Note the one earlier datapoint from the *AER*: a mean time of 13.5 months in 1979. To those who may be puzzling over the figure I would like to confirm that Clower reported in his 1982 editor's report that for the previous three issues his mean submit-accept time was less than two months and his mean time to rejection for rejected papers was 25 days. This seems quite remarkable before the advent of e-mail and fax machines, especially given that in 1983 Clower reports receiving help from 550 referees. Clower indicates that he received a great deal of positive feedback from authors, but also enough hate mail that he felt obliged to share his favorite ("should you learn the date in advance I should be pleased to be present at

### Total Review Time at General Interest Journals: 1970 - 1999

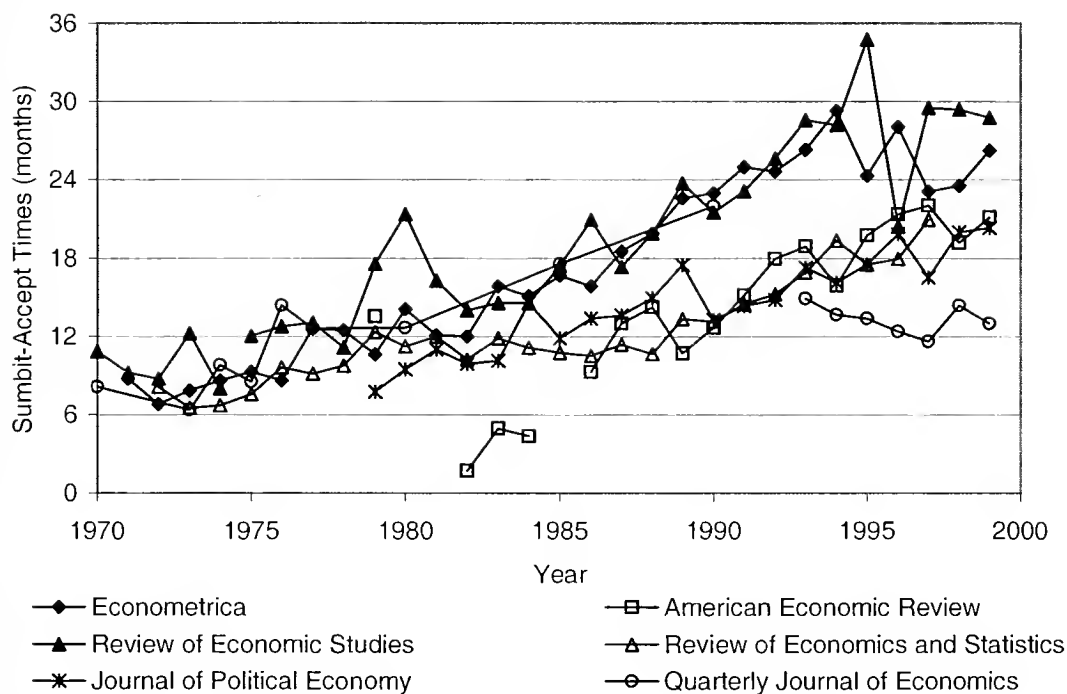


Figure 1: Changes in total review times at top general interest journals

The figure graphs the mean length of time between submission and acceptance for papers published in six general interest journals between 1970 and 1999. The data for *Econometrica* and the pre-1975 data for *Review of Economic Studies* do not include the length of time between the resubmission of the final version of a paper and acceptance. Data for the *AER* and *JPE* include all papers and are taken from annual editors reports. Data for the other journals is tabulated from records on individual papers and omits shorter papers, notes, comments, replies, etc.

similar pattern up through 1990, but with the change of the editorial staff in 1991 there was a clear break in the trend and mean total review times have now dropped to about a year. I will discuss below the ways in which the *QJE* is and is not an exception to the pattern of the other journals.

The slowdown of the publishing process illustrated above is not restricted to the top general interest journals. Similar patterns are found throughout the field journals and in finance. Table 1 reports mean total review times for various journals in 1970, 1980, 1990 and 1999.<sup>9</sup> Ellison (2000) provides a broader overview of where the pattern is and is not found in other disciplines in the social, natural and mathematical sciences.<sup>10</sup>

In the discussion above, I've focused on changes in mean submit-accept times. When one looks at the distribution of submit-accept times, the uniformity of the slowdown can be striking. Figure 2 provides one (admittedly extreme) example. The figure presents histograms of the submit-accept times for papers published in the *Review of Economic Studies* in 1975 and 1995. In 1975 the modal experience was to have a paper accepted in four to six months and seventy percent of the papers were accepted within a year. In 1995 almost nothing was accepted quickly. Only three of the twenty eight papers were accepted in less than sixteen months. The majority of the papers are in the sixteen to thirty two month range, and there is also a substantial set of papers taking from three to five years.

## 2.2 Where is the increase occurring?

A common first reaction to seeing the figures on the slowdown of submit-accept times is to imagine that the story is one of a breakdown of norms for timely refereeing. Everyone has heard horror stories about slow responses and it is easy to imagine papers just sitting for longer and longer periods in piles on referees' desks waiting to be read. Upon further reflection, it is obvious that this cannot be the whole story -- the increases in submit-accept times are too large to be due to a single round of slow refereeing.<sup>11</sup>

Figure 3 suggests that, in fact, slow refereeing is just a small part of the story. The figure illustrates how the mean time between submission and the sending of an initial decision letter has changed over time at four of the top five general interest journals.<sup>12</sup> At your hanging") in his first editor's report.

<sup>9</sup>The definition of total review time and the years used varies across journals as explained in the table notes.

<sup>10</sup>Ellison (2000) also gives a cross-field view of the trend toward writing longer papers with more references.

<sup>11</sup>See Hamermesh (1994) for a discussion of the distribution of refereeing times at several journals.

<sup>12</sup>The set of papers included in the calculation varies somewhat from journal to journal so the figures should not be compared across journals. Details are given in the notes to the figure.

Table 1: Changes in review times at various journals

Journal	Mean total review time in year			
	1970	1980	1990	1999
Top five general interest journals				
<i>American Economic Review</i>		<sup>a</sup> 13.5	12.7	
<i>Econometrica</i>	<sup>b</sup> 8.8	<sup>b</sup> 14.0	<sup>b</sup> 22.9	<sup>b</sup> 26.3
<i>Journal of Political Economy</i>		9.5	13.3	20.3
<i>Quarterly Journal of Economics</i>	8.1	12.7	22.0	13.0
<i>Review of Economic Studies</i>	<sup>b</sup> 10.9	21.5	21.2	28.8
Other general interest journals				
<i>Canadian Journal of Economics</i>		<sup>a</sup> 11.3		16.6
<i>Economic Inquiry</i>		<sup>a</sup> 3.4		13.0
<i>Economic Journal</i>		<sup>a</sup> 9.5		<sup>b</sup> 18.2
<i>International Economic Review</i>	<sup>b</sup> 7.8	<sup>b</sup> 11.9	<sup>b</sup> 15.9	<sup>b</sup> 16.8
<i>Review of Economics and Statistics</i>	8.1	11.4	13.1	18.8
Economics field journals				
<i>Journal of Applied Econometrics</i>			<sup>b</sup> 16.3	<sup>b</sup> 21.5
<i>Journal of Comparative Economics</i>		<sup>b</sup> 10.3	<sup>b</sup> 10.9	<sup>b</sup> 10.1
<i>Journal of Development Economics</i>	<sup>bc</sup> 5.6	<sup>b</sup> 6.4	<sup>b</sup> 12.6	<sup>b</sup> 17.3
<i>Journal of Econometrics</i>		<sup>b</sup> 9.7	<sup>b</sup> 17.6	<sup>b</sup> 25.5
<i>Journal of Economic Theory</i>	<sup>b</sup> 0.6	<sup>b</sup> 6.1	<sup>b</sup> 17.0	<sup>b</sup> 16.4
<i>Journal of Environmental Ec. &amp; Man.</i>		<sup>b</sup> 5.5	<sup>b</sup> 6.6	<sup>b</sup> 13.1
<i>Journal of International Economics</i>		<sup>a</sup> 8.7		16.2
<i>Journal of Law and Economics</i>		<sup>a</sup> 6.6		14.8
<i>Journal of Mathematical Economics</i>	<sup>bc</sup> 2.2	<sup>b</sup> 7.5	17.5	8.5
<i>Journal of Monetary Economics</i>			<sup>b</sup> 11.7	<sup>b</sup> 16.0
<i>Journal of Public Economics</i>	<sup>bd</sup> 2.6	<sup>b</sup> 12.5	<sup>b</sup> 14.2	<sup>b</sup> 9.9
<i>Journal of Urban Economics</i>		<sup>b</sup> 5.4	<sup>b</sup> 10.3	<sup>b</sup> 8.8
<i>RAND Journal of Economics</i>		<sup>a</sup> 7.2	20.0	20.9
Journals in related fields				
<i>Accounting Review</i>		10.1	20.7	14.5
<i>Journal of Accounting and Economics</i>		<sup>b</sup> 11.4	<sup>b</sup> 12.5	<sup>b</sup> 11.5
<i>Journal of Finance</i>		<sup>a</sup> 6.5		18.6
<i>Journal of Financial Economics</i>	<sup>bc</sup> 2.6	<sup>b</sup> 7.5	<sup>b</sup> 12.4	<sup>b</sup> 14.8

The table records the mean time between initial submission and acceptance for articles published in various journals in various years. Notes: *a* - Data from Yohe (1980) is for 1979 and probably does not include the review time for the final resubmission. *b* - Does not include review time for final resubmission. *c* - Data for 1974. *d* - Data for 1972.

Distribution of Submit-Accept Times  
 Review of Economic Studies  
 1975 & 1995

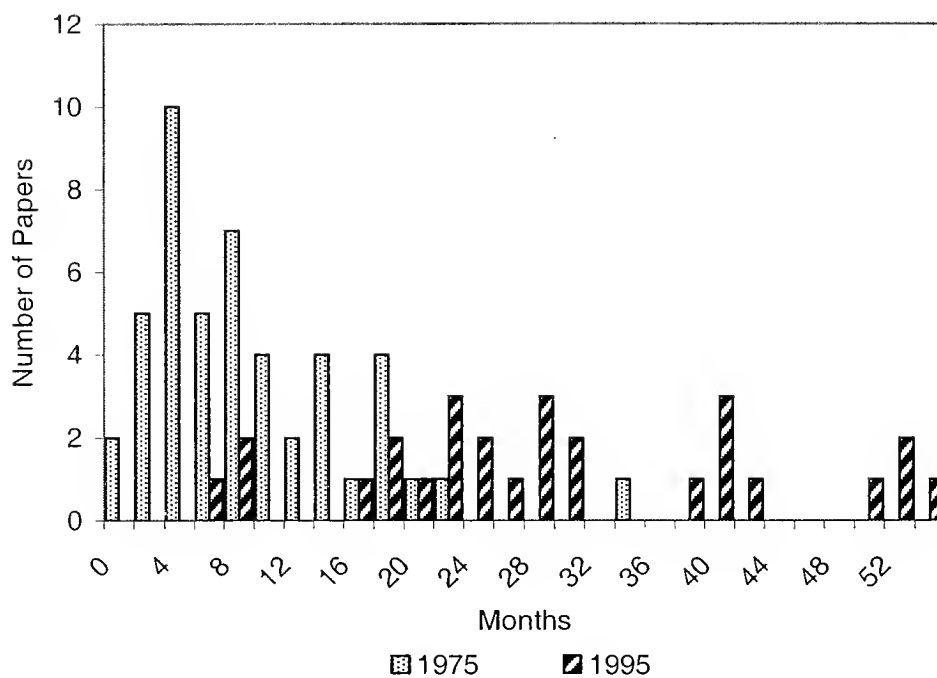


Figure 2: The distribution of submit-accept times at the *Review of Economic Studies*: 1975 and 1995

The figure contains a histogram of the time between submission and acceptance for articles published in the *Review of Economic Studies* in 1975 and 1995. One 1995 observation at 84 months was omitted to facilitate the scaling of the figure.

*Econometrica*, the mean first response time in the late nineties is virtually identical to what it was in the late seventies. At the *JPE* the latest figure is about two months longer than the earliest; this is about twenty percent of the increase in review times between 1982 and 1999. The *AER* shows about a one-and-a-half month increase since 1986; this is about 15 percent as large as the increase in submit-accept times over the same period.<sup>13</sup> A discussion of what may in turn have caused first responses to slow down must take into account that the time a referee spends working on a report is small relative to the amount of time the paper sits on his or her desk. I would imagine that the biggest causes of changes in first response times are changes in the total demands on referees and changes in social norms about acceptable delays. To the extent that referees wait until they have a sufficiently large block of time free to complete a report before starting the task, some part of the slowdown in first responses could also be due to increases in the complexity of papers and/or the increases in how substantial a referees' suggestions for improvement are expected to be.

The pattern at the *QJE* is different from the others. The *QJE* experienced a dramatic slowdown of first responses between 1970 and 1990, followed by an even more dramatic speed up in the 1990's.<sup>14</sup> It is this difference (and reviewing many revisions quickly without using referees) that accounts for the *QJE*'s unique pattern of submit-accept times.

Assuming that the data on mean first response times are also representative of what has happened at other journals and in earlier time periods, the majority of the overall increase in submit-accept times must be attributable to one or more of four factors: an increase in the number of times papers are being revised; an increase in the length of time authors take to make revisions; an increase in the mean review time for resubmissions; and a growing disparity between mean review times and mean review times for accepted papers. I now discuss each of these factors.

Evidence from a variety of sources indicates that papers are now revised much more often and more extensively than they once were. First, while older economists I interviewed uniformly indicated that journals have required revisions for as long they could remember, they also indicated that having papers accepted without revisions was not uncommon, that revisions often focused just on expositional (or even grammatical) points, and that requests

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<sup>13</sup>Again, the figures from the Clower era are almost surely not representative of what happened earlier and are probably best ignored.

<sup>14</sup>Larry Katz has turned in the most impressive performance. His mean first response time is 39 days, and none of the 1494 papers I observe him handling took longer than six months and one week. I have not included estimates of mean first response times for the *QJE* between 1980 and 1990 because the increasing slowdown of the late eighties was accompanied by recordkeeping that was increasingly hard to follow. Table 4 provides a related measurement that gives some feel for the severe delays of the late eighties.

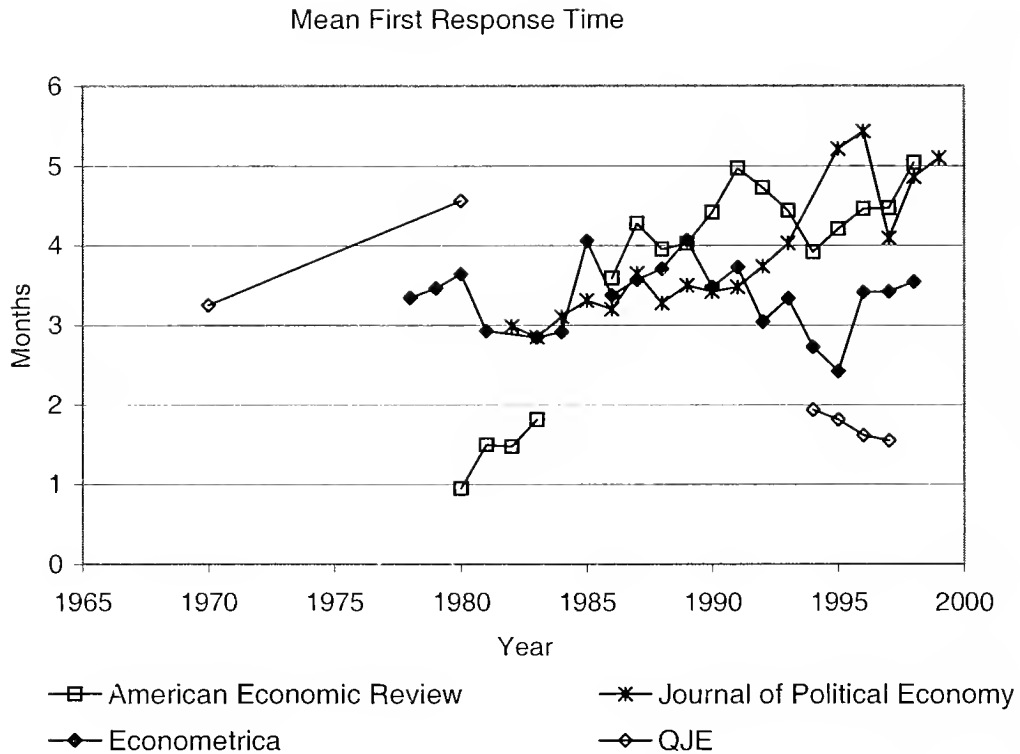


Figure 3: Changes in first response times at top journals

The figure graphs the mean length of time between submission of a manuscript to each of four general interest journals and the journal reaching an initial decision. The *Econometrica* data is an estimate of the mean first response time for all submissions (combining new submissions and resubmissions) derived from data in the editors' reports on papers pending at the end of the year under the assumptions that papers arrive uniformly throughout the year and no paper takes longer than twelve months. The data for year  $t$  is the mean first response time for submissions arriving at *Econometrica* between July 1st of year  $t - 1$  and June 30th of year  $t$ . Figures for the *AER* are estimated from histograms of response times in the annual editor's reports and relate to papers arriving in the same fiscal year as for *Econometrica*. Figures for the *JPE* are obtained from journal annual reports. They appear to be the mean first response time for papers that are rejected on the initial submission in the indicated year. The 1970 and 1980 *QJE* numbers are the mean first response time for a random sample of papers with first responses in the indicated year. Figures for the *QJE* for 1994 to 1997 are the mean for all papers with first responses in the indicated year.



for substantial changes were sometimes regarded as unreasonable unless particular problems with the paper had been identified.<sup>15</sup>

Second, I obtained quantitative evidence on the growth of revisions by reading through old index card records kept by the *QJE*.<sup>16</sup> The first row of Table 2 extends the timespan of our view of the slowdown, and indicates that at the *QJE* the slowdown begins around 1960 following a couple decades of constant review times.<sup>17</sup> The second row of Table 2 illustrates that (despite the *QJE* being an exception to the rule of increasing total review times) the mean number of revisions authors were required to make was roughly constant at around 0.6 from 1940 to 1960, and then increased steadily to a level of about 2.0 today.

A striking observation from the old *QJE* records is that the *QJE* used to have four categories of responses to initial submissions rather than two — papers were sometimes accepted as is and “accept-but-revise” was a separate category that was more common than “revise-and-resubmit.” Of the articles published in 1960, for example, 12 were accepted on the initial submission, 11 initially received an accept-but-revise and 5 a revise-and-resubmit.<sup>18</sup> Marshall’s (1959) discussion of a survey of twenty-six journal editors suggests that the *QJE*’s practice of almost always making up or down decisions on initial submissions (but sometimes using the accept-but-revise option) was the norm. Marshall never mentions the possibility of a revise-and-resubmit and says

The writer who submits a manuscript will normally receive fairly prompt notice of an acceptance or rejection. Twenty-three [of 26] editors reported that they gave notification one way or the other within 1 to 2 months, and only 2 editors reported a time-lag of as much as 4 months or more. . . . The waiting period between the time of acceptance and appearance in print can also be explained in part by the necessity felt by many editors of having authors make extensive revisions. Eighteen of the editors reported that major revisions were frequently

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<sup>15</sup>An indirect source of evidence I’ve found amusing is looking at the organization of journals’ databases. The *JPE* database, for example, was only designed to allow for information to be recorded on up to two revisions and the editorial staff have had to improvise methods (including writing over the data on earlier revisions and entering data into a “comments” field) for keeping track of the now not uncommon third and further revisions.

<sup>16</sup>The last two columns of the table are derived from the *QJE*’s next-to-current computer database.

<sup>17</sup>The fact that it took only three to four months to accept papers in the 1940’s seems remarkable today given the handicaps under which the editors worked. One example that had not occurred to me until reading through the records is that requests for multiple reports on a paper were done sequentially rather than simultaneously — there were no photocopy machines and the journal had to wait for the first referee to return the manuscript before sending it to the second.

<sup>18</sup>The 1970 breakdown was 3 accepts, 12 accept-but-revises, 9 revise-and-resubmits, and 1 reject (which the author protested and eventually overturned on his third resubmission).

necessary.<sup>19</sup> (p. 137)

The third row of the Table 2 illustrates that the growth in revisions at the *QJE* is even more dramatic if one does not count revisions that occurred after a paper was already accepted.

Table 2: Patterns of revisions over time at the *Quarterly Journal of Economics*

	Year of publication								
	1940	1950	1960	1970	1980	1985	1990	1995	1997
Mean submit-accept time (months)	3.7	3.8	3.6	8.1	12.7	17.6	22.0	13.4	11.6
Mean number of revisions	0.6	0.8	0.6	1.2	1.4	1.5	1.7	2.2	2.0
Mean # of revisions before acceptance	0.4	0.1	0.2	0.5	0.8	1.0	1.7	2.2	2.0
Mean author time for first preaccept revision (months)	1.4	2.1	2.0	2.1	3.0	4.2	3.6	4.1	4.7

The table reports statistics on the handling of articles (not including notes, comments and replies) published in the *QJE* in the indicated years. The first row is the mean total time between submission and final acceptance (including time spent waiting for and reviewing revisions to papers which had received an “accept-but-revise” decision). The second is the mean number of revisions authors made. The third is the same, but only counting revisions that were made prior to any acceptance letter being sent (including “accept-but-revise”). The fourth is the mean time between an author being sent a “revise-and-resubmit” letter for the first time on a paper and the revision arriving at the journal office.

Data on the breakdown of total submit-accept times at the *JPE* provides some indirect evidence on the growth of revisions. Table 3 records for each year since 1979 the mean submit-accept time at the *JPE* and the breakdown of this time into time with the editors awaiting a decision letter, time with the authors being revised and time spent waiting for referees’ reports. The amount of time papers spend with the editors has increased dramatically from about two months in 1979 - 1980 to more than seven in the most recent years. Some of this increase may be due to editors devoting less effort to keeping up with

<sup>19</sup>Marshall’s (1959) use of the term “major revision” is clearly different from how it would be understood today. The time necessary for authors to make these revisions and for journals to approve them are part of the acceptance-publication lags in his data. While he estimates that journals need “about 3 months to ‘produce’ an issue after all of the editorial work on it has been completed” and papers undoubtedly spend two or more months on average waiting in the queue for the next available slot in the journal (the delay would be one-and-a-half months on average at a quarterly journal even if there were no backlog at all), only ten of the twenty-six journals in his sample had lags between acceptance and publication of 7 months or more.

the flow of papers. The total amount of time a paper spends with the editors, however, is the product of amount of time a paper spends with the editors on each round and the number times it is revised. My guess would be that a substantial portion of the increase is attributable to the average number of rounds having increased. Again, part of the increase may also reflect editors waiting longer to write letters because they must clear a larger block of time to contemplate longer referee reports, to describe more extensive revisions, and/or to evaluate more substantial revisions.

Table 3: A breakdown of submit-accept times for the *Journal of Political Economy*

Breakdown of mean submit-accept time	Year of publication										
	1979	1980	1981	1982	1983	1984	1985	1986	1987	1988	1989
Total time	7.8	9.5	11.0	9.9	10.1	14.5	11.8	13.4	13.6	15.0	17.4
with editors	1.8	2.3	3.2	3.4	3.4	4.8	4.9	4.6	5.0	6.4	4.7
with authors	3.3	4.1	4.4	4.3	3.8	5.5	3.9	5.1	4.9	4.7	7.1
with referees	2.7	3.1	3.4	2.2	2.9	4.3	3.0	3.7	3.7	3.8	4.6
	Year of publication										
	1990	1991	1992	1993	1994	1995	1996	1997	1998	1999	
Total time	13.3	14.3	14.8	17.3	16.1	17.5	19.8	16.5	20.0	20.3	
with editors	3.6	4.2	4.4	5.8	6.5	6.1	7.4	6.8	8.4	7.4	
with authors	4.9	6.1	6.0	6.5	4.7	6.5	7.5	3.9	6.7	6.6	
with referees	4.8	4.0	4.3	4.9	4.9	5.2	5.0	5.8	5.0	6.2	

The table reports the mean submit-accept time in months and two components of this time for papers published in the *JPE* in the indicated years. The figures were obtained from annual reports of the journal.

The data on submit-accept times at the top finance journals (some of which is in Table 1) provides another illustration of a trend toward more revisions. While the *Journal of Financial Economics* is rightfully proud of the fact that its median first response time in 1999 was just 34 days (as it was when first reported in 1976), the trend in the journal's mean submit-accept times is much like those at top economics journals. Mean submit-accept times have risen from about 3 months in 1974 to about 15 months in 1999.<sup>20</sup> Similarly, the *Journal of Finance* had a median turnaround time of just 41 days in 1999, but its mean submit-accept time has risen from 6.5 months in 1979 to 18.6 months in 1999.<sup>21</sup>

<sup>20</sup>The *JFE* only reports submission and final resubmission dates. The mean difference between these was 2.6 months in 1974 (the journal's first year) and 14.8 months in 1999. Fourteen of the fifteen papers published in 1974 were revised at least once.

<sup>21</sup>The distribution of submit-accept times at the *JF* is skewed by the presence of a few papers with very long lags, but the median is still 15 months. Papers that ended up in its shorter papers section had an even

A second factor contributing to the increase in submit-accept times is that authors are taking longer to revise their papers. The best data source I have on this is again the *QJE* records. The final row of Table 2 reports the mean time in months between the issuance of a “revise-and-resubmit” letter in response to an initial submission and the receipt of the revision for papers published in the indicated year.<sup>22</sup> The time spent doing first revisions has increased steadily since 1940. Authors were about one month slower in 1980 than in 1970 and about one and a half months slower in the mid 1990s than in 1980. How much of this is due to authors being asked to do more in a revision and how much is due to authors simply being slower is impossible to know given the data limitations. The fact that authors of the 1940 papers that were revised took only 1.4 months on average to revise their manuscripts (including the time needed to have them retyped and waiting time for the mail in both directions) suggests that the revisions must have been less extensive than today’s. The other source of information on authors’ revision times available to me is the data from the *JPE* in Table 3. This data mixes together increases in the time authors spend per revision and increases in the number of revisions authors are asked to make. There is a lot of variability from year to year, but the total time authors take revising seems to have increased by about two and a half months since 1980.

While journals are only taking a little longer to review initial submissions, my impression is that they are taking much longer to review resubmissions (although I lack data on this). I do not, however, think of this as a fundamental cause of the slowdown. Instead, I think of it as a reflection of the fact that first resubmissions are no longer thought of as final resubmissions. My guess is that review times for final resubmissions have not changed much.

A final possibility is that increases in first review times are a larger portion of the overall increase in submit-accept times than is suggested by the data in Figure 3. Mean first response times for accepted papers can be substantially different from the mean first responses for rejected papers. Table 4 compares the first response time conditional on eventual acceptance to more standard “unconditional” measures at the *QJE* and *JPE*.<sup>23</sup> At the *QJE* the two series have been about a month apart since 1970, and it does not appear that there are any trends in the difference between the two series. At the *JPE* the differences

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longer lag: 23.2 months on average.

<sup>22</sup>I do not include in the sample revisions which were made in response to “accept-but-revise” letters.

<sup>23</sup>In recent years a substantial number of submissions to the *QJE* have been rejected without using referees. To provide a more accurate picture of trends in referees’ evaluation times I do not include the (very fast) first response times for such papers from the *QJE* data for the years after 1993.

are much larger. While only recent data is available, slower mean first response times are definitely a significant part of the overall slowdown. For papers published in 1979, the mean submit-accept time was 7.8 months. This number includes an average of 3.3 months that papers spent on authors' desks being revised, so the mean first response time conditional on acceptance could not have been greater than 4.5 months and was probably at least a month shorter. For papers published in 1995, the mean submit-accept time was 17.5 months and the mean first response time was 6.5 months. Hence, the lengthening of the first response probably accounts for at least one-quarter of the 1979-1995 slowdown.<sup>24</sup>

Table 4: First response times for accepted and other papers

Sample of papers	Mean first response time in months									
	1970	1980	1985	1990	1992	1993	1994	1995	1996	1997
<i>QJE</i> : sent to referees	3.3	4.6					3.5	3.2	2.9	2.7
<i>QJE</i> : accepted	4.8	5.8	7.2	9.0			4.8	3.7	3.2	3.7
<i>JPE</i> : rejected			3.3	3.4	3.7	4.0		5.2	5.4	4.1
<i>JPE</i> : accepted					6.9	6.7	6.9	8.4	10.3	7.8

The table presents various mean first response times. The first row gives estimated means (from a random sample) for papers (including those rejected without using referees) with first responses in 1970 and 1980 and the true sample mean for all papers with first responses in 1994 - 1997 (not including those rejected without using referees) by the *QJE*. The second row gives mean first response times for papers that were eventually accepted. For 1970 - 1990 the means are for papers published in the indicated year; for 1994 - 1997 numbers are means for papers with first responses in the indicated year and accepted prior to August of 1999. The third row gives mean first response times for papers that were rejected on the initial submission by the *JPE* in the indicated year. The fourth row gives the mean first response time for papers with first responses in the indicated year that were accepted prior to January of 1999.

Overall, I would conclude that some fraction of the slowdown in the publishing process (perhaps a quarter at the *JPE*) is due to slower first responses. A larger part of the slowdown appears to be attributable to a practice of asking authors to make more and larger revisions to their papers.

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<sup>24</sup>For papers published in 1997, the mean submit-accept time was 16.5 months and the mean first-response time was 9.8 months — the majority of the 1980-1997 slowdown may thus be attributed to slower first responses. It appears, however, that 1997 is outlier. One editor was very slow and the journal may have responded to slow initial turnarounds by shortening and speeding up the revision process.

### 3 Costs and benefits of revisions

I now turn to the task of evaluating a number of potential explanations for the trends discussed in the previous section. I begin with a simple set of arguments focusing on direct changes in the costs and benefits of revising papers.

#### 3.1 The potential explanation

The arguments I consider here are of the form: “Over the last three decades exogenous change  $X$  has occurred. This has reduced the marginal cost to authors of making revisions and/or increased the marginal benefit to the profession of having papers revised more extensively. Hence it is now optimal to have longer submit-accept times.” The two environmental changes that seem most compelling as the  $X$  are improvements in computer software and changes in how economics papers are disseminated.

Thirty years ago there were no microcomputers. Rudimentary word processing software was available on mainframes in the 1960’s, but until the late seventies or early eighties revising a paper extensively usually entailed having it retyped.<sup>25</sup> Running regressions was also much more difficult. While some statistical software existed on mainframes earlier, statistical packages, as we now understand the term, mostly developed during the 1970’s.<sup>26</sup> The first spreadsheet, Visicalc, appeared in 1979. Statistical packages for microcomputers appeared in the early eighties and were adopted very quickly. The new software must have reduced the cost of revising papers. It seems reasonable to suppose that journals may have increased the number of revisions they requested as an optimal response. This might or might not be expected to lead to an increase in the amount of time authors spend revising papers (depending on whether the increased speed with which they can make revisions offsets their being asked to do more), but would result in journals spending more time reviewing the extra revisions.

Thirty years ago most economists would not hear about new research until it was published in journals. Now, with widely available working paper series and web sites, it can be argued that journals are less in the business of disseminating information and more in the business of certifying the quality of papers. This makes timeliness of publication less important and may have led journals to slow down the process and evaluate papers more carefully. Even expositional issues can become more important: as long as the version that thirty years ago would have appeared as the published version is now available as a working

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<sup>25</sup>Smaller revisions were often accomplished by cutting and pasting.

<sup>26</sup>For example, the first version of SAS (for mainframes running MVS/TSO) appeared in 1976.

paper readers are made unambiguously better off by delays to improve exposition. Those who want to see the paper right away can look at the working paper and those who prefer to wait for a more clearly explicated version (or who do not become interested until later) will benefit from reading a clearer paper.

### 3.2 Evidence

While the stories above are plausible, I've found little evidence to support them. First, I've discussed the slowdown with editors or former editors of all of the top general interest journals (and editors of a number of field journals) and none mentioned to me that increasing the number of rounds of revision or lengthening the review process was a conscious decision. Instead, even most long-serving editors seemed unaware that there had been substantial changes in the length of the review process. A few editors indicated that they felt that reviewing papers carefully and maintaining high quality standards is a higher priority than timely publication and this justifies current review times, but this view was not expressed in conjunction with a view that the importance of high standards has changed. Overwhelmingly, editors indicated that they handle papers now as they always have.

Annual editor's reports provide a source of contemporary written records on editors' plans. At the *AER*, most of the editor's reports from the post-Clower era simply note that the mean time to publication for accepted papers is about what it was the year before. These observations are correct and given that the tables only contain one year of data it is probably not surprising that there is no evident recognition that when one aggregates the small year-to-year changes they become a large event. No motivation for lengthening the review process is mentioned. The standard table format in the unpublished *JPE* editors' reports includes three to five years of data on submit-accept times. Perhaps as a result the *JPE* reports do show a recognition of a continuing slowdown (although not of its full long-run magnitude.) The editors' comments do not suggest that the slowdown is planned or seen as optimal. For example, the 1981 report says,

The increase in the time from initial submission to final publication of accepted papers has risen by 5 months in the past two years, a most unsatisfactory trend. . . . The articles a professional journal publishes cannot be timely in any short run sense, but the reversal of this trend is going to be our major goal.

The 1982, 1984 and 1988 reports express the same desire. Only the 1990 report has a different perspective. In good Chicago style it recognizes that the optimal length of the

review process must equate marginal costs and benefits, but takes no position on what this means in practice:

Is this rate of review and revision and publication regrettable? Of course, almost everyone would like to have his or her work published instantly, but we believe that the referee and editorial comments and the time for reconsideration usually lead to a significant improvement of an article. A detailed comparison of initial submissions and printed versions of papers would be a useful undertaking: would it further speed the editors or teach the contributors patience?

A second problem with the cost and benefit explanations I've mentioned is that they do not seem to fit well with the timing of the slowdown, which I take to be a gradual continuous change since about 1960. For example, the period from 1985 to 1995 had about as large a slowdown as any other ten year period. Software can't really account for this, because word processors and statistical packages had already been widely adopted by the start of the period.<sup>27</sup> Web-based paper distribution was not important in 1995 and paper working paper series had been around for a long time before 1985.<sup>28</sup> Another question that is hard to answer with the cost and benefit explanations is why review times (especially for theory papers) started to lengthen around 1960.

One question on which I can provide some quantitative evidence is the difference in trends for theoretical and empirical papers. Since revising empirical papers has been made easier both by improvements in word processing and by improvements in statistical packages, the cost of revision argument suggests that empirical papers may have experienced a greater slowdown than theory papers.

I have data on submit-accept times (or submit-final resubmit times) for over 5500 articles published since 1970. This includes most articles published in *Econometrica*, *REStud* and *REStat*, papers published in the *JPE* and *AER* in 1993 or later, papers published in the *QJE* in 1973-1977, 1980, 1985, 1990 or since 1993, and papers in the *RAND Journal of Economics* since 1986. The data stop at the end of 1997 or the middle of 1998 for all journals. I had research assistants inspect more than two thousand of the papers and classify them as theoretical or empirical.<sup>29</sup> For the rest of the papers I created an estimated classification by defining a continuous variable, *Theory*, to be equal to the mean of the theory dummies

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<sup>27</sup>Later improvements have incorporated new features and make papers look nicer, but have not fundamentally changed how hard it is to make revisions.

<sup>28</sup>For example, the current NBER working paper series started in 1973.

<sup>29</sup>The set consists of most papers in the 1990's and about half of the 1970's papers.



of papers with the same JEL code for which I had data.<sup>30</sup>

One clear fact in the data is that authors of theoretical papers now face a longer review process. In my 1990's subsample I estimate the mean submit-accept time for theoretical papers to be 22.5 months and the mean for empirical papers to be 20.0 months. This should not be surprising. We have already seen that *Econometrica* and *REStud* have longer review processes than the other journals and these journals publish a disproportionate share of theoretical papers. If one views differences across journals as likely due to idiosyncratic journal-specific factors and asks how review times differ within each journal, the answer is that there are no large differences. In regressions with journal fixed effects, journal specific trends and other control variables, the *Theory* variable is insignificant in every decade.<sup>31</sup> Certainly, there is no evidence of a more severe slowdown for empirical papers.

Overall, I feel that there is little evidence to suggest that the slowdown is an optimal response to changes in the costs of revisions and the benefits of timely publication.

## 4 Democratization

I use the term “democratization” to refer to the idea that the publishing process at top journals may have become more open and meritocratic over time.<sup>32</sup> For a number of reasons, such a shift might lead to a lengthening of the review process. In this section, I examine these explanations empirically. I find little evidence that a democratization has taken place, and also find little evidence of cross-sectional patterns that would be expected if the slowdown were linked to democratization.

### 4.1 The potential explanation

The starting point for democratization explanations for the slowdown is an assumption that in the “old days”, economics journals were more of an old-boys network and were less concerned with carefully evaluating the merits of submissions than they are today.<sup>33</sup> There are a number of reasons why such a shift might lead to a slowdown.

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<sup>30</sup>On average 83% of papers in a JEL code have the modal classification.

<sup>31</sup>Looking journal-by-journal in the 1990's, theory papers have significantly shorter review times at the *AER* (the coefficient estimate is -140 days with a t-statistic of 3.0) and at least moderately significantly longer review times at *Econometrica* (coef. est. 120, t-stat. 1.8) and *RAND* (coef. est. 171, t-stat. 2.3). See Section 4.2 for a full description of the regressions.

<sup>32</sup>Such a change could have occurred in response to changes in general societal norms, because of an increased fear of lawsuits or for other reasons.

<sup>33</sup>Certainly some aspects of the process in the old days look less democratic. For example, in the 1940's the *QJE* editorial staff kept track of referees using only initials. Presumably this was sufficient because most (or all) of the referees were in Littauer.

First, carefully reading all of the papers that are submitted to a top economics journal is a demanding task. If in some earlier era editors did not evaluate papers as carefully and instead accepted papers by famous authors (or their friends), all papers could be reviewed more quickly.

A democratization could also lead to higher mean submit-accept times by lengthening review times for some authors and by changing the composition of the pool of accepted papers. An example of an effect of the first type would be that authors who previously enjoyed preferential treatment would presumably face longer delays. A more open review process might change the composition of top journals, for example, by allowing more authors from outside top schools or from outside the U.S. to publish and by reducing the share of privileged authors. Authors who are not at top schools may have longer submit-accept times because they have fewer colleagues able to help them improve their papers prior to submission and because they are less able to tailor their submissions to editors' tastes. Authors who are not native English speakers may have longer submit-accept times because they need more editorial input at the end of the process to improve the readability of their papers.

## 4.2 Evidence on democratization

I examine the idea that a democratization of the publication process has contributed to the slowdown in two main steps: first looking at whether there is any evidence that publication has become more democratic over the period and then looking for evidence of connections between democratization and submit-accept times.

### 4.2.1 Has there been a democratization? Evidence from the characteristics of accepted papers

The first place that I'll look for quantitative evidence on whether the process has become more open and meritocratic since 1970 is in the composition of the pool of accepted papers. A natural prediction is that a democratization of the review process (especially in combination with the growth of the profession) should reduce the concentration of publication.<sup>34</sup> The top  $x$  percent of economists would presumably capture a smaller share of publications in top journals as other economists are more able to compete with them for scarce space,

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<sup>34</sup>Of course this need not be true. For example it could be that the elite received preferential treatment under the old system but were writing the best papers anyway, or that more meritocratic reviews simply lead to publications being concentrated in the hands of the best authors instead of the most famous authors. A possibility relevant to school-level concentration is that the hiring process at top schools may have become more meritocratic and led to a greater concentration of talent.

and economists at the top  $N$  schools would presumably see their share of publications decline as economists from lower ranked institutions are able to compete on a more equal footing and grow in number.

The first two rows of Table 5 examine changes over time in the author-level and school-level concentration of publication in top general interest journals. The first row gives the herfindahl index of authors' "market shares" of all articles in the top five general interest journals in each decade, i.e. it reports  $\sum_a s_{at}^2$  where  $s_{at}$  is the fraction of all articles in decade  $t$  written by author  $a$ .<sup>35</sup> A smaller value of the herfindahl index indicates that publication was less concentrated. The data indicate that there was a small increase in concentration between the 1970's and the 1980's and then a small decline between the 1980's and 1990's. Despite the growth of the profession, the author-level concentration of publication in the 1990's is about what it was in the 1970's.

Table 5: Trends in authorship at top five journals

	Decade				
	1950's	1960's	1970's	1980's	1990's
Author-level herfindahl			.00135	.00148	.00133
Percent by top 8 schools	36.5	31.8	27.2	28.2	33.8
Harvard share of <i>QJE</i>	14.5	12.3	12.7	6.4	12.5
Chicago share of <i>JPE</i>	15.6	10.6	11.2	7.0	9.4
Non-English name share			26.3	25.2	30.6
Percent female			3.5	4.5	7.5

The first row of the table reports the herfindahl index of author's share of articles in five journals: *AER*, *Econometrica*, *JPE*, *QJE* and *REStud*. The second row gives the percent of weighted pages in the *AER*, *JPE*, and *QJE* by authors from the top eight schools for that decade. The third and fourth rows are percentages of pages with fractional credit given for coauthored articles. The fifth and sixth rows give the percent of articles in the top five journals written by authors with first names which were classified as indicating that the author was a non-native English speaker and a woman, respectively.

While my data do not include authors' affiliations for pre-1989 observations, I can examine changes in the school-level concentration of publication by comparing data for the 1990's with numbers for earlier decades reported by Siegfried (1994).<sup>36</sup> The second row

<sup>35</sup>Note that here I am able to make use of all articles that appeared in the *AER*, *Econometrica*, *JPE*, *QJE*, and *REStud* between 1970 and some time in 1997-1998. Each author is given fractional credit for coauthored articles. I include only regular articles, omitting where I can shorter papers, notes, comments, etc. as well as articles in symposia or special issues, presidential addresses, etc.

<sup>36</sup>Some of the numbers in Siegfried (1994) were in turn directly reprinted from Cleary and Edwards (1960), Yotopoulos (1961) and Siegfried (1972).

of Table 5 reports the weighted fraction of pages in the *AER*, *QJE*, and *JPE* written by authors from the top eight schools.<sup>37</sup> The numbers point to an increase in school-level concentration, both between the 1970's and the 1980's and between the 1980's and the 1990's.<sup>38</sup> I have included the earlier decades in the table because I thought that they suggest a reason why the impression that the profession has opened up since the "old days" is fairly widespread. There was a substantial decline in the top eight schools' share of publications between the 1950's and the 1970's.

The remainder of Table 5 examines other trends that may relate to democratization. Rows 3 and 4 also piggyback on Siegfried's (1994) work to examine trends in the (page-weighted) share of articles in the *JPE* and *QJE* written by authors at the journal's home institution. In each case the substantial decline between the 1970's and the 1980's noted by Siegfried was followed by a substantial increase between the 1980's and the 1990's. As a result, the *QJE* has about the same level of Harvard authorship in the 1990's as in the 1970's, while the *JPE* has somewhat less of a Chicago concentration. While the *JPE* trend could be taken as indicative of a democratization of the *JPE*, the fact that the combined share of *AER*, *QJE* and *JPE* pages by Chicago authors has declined only slightly between the 1970's and 1990's suggests that it is more likely attributable to an increase in Chicago economists' desire to publish in the *QJE*.<sup>39</sup>

The final two rows of the table report estimates of the fraction of articles in the top five general interest journals written by non-native English speakers and by women. The estimates for all three decades were obtained by classifying authors on the basis of their first names.<sup>40</sup> Each group has increased their share of publications, but as a fraction of the

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<sup>37</sup>Following Siegfried the "top eight" is defined to be the eight schools with the most pages in the three journals in the decade. For the 1990's this is Harvard, Chicago, MIT, Princeton, Northwestern, Stanford, Pennsylvania and UC-Berkeley. Differences between this calculation and other calculations I've been carrying out include that it does not include publications in *Econometrica* and *REStud*, that it is based on page-lengths not numbers of articles (with pages weighted so that *JPE* and *QJE* pages count for 0.707 and 0.658 *AER* pages, respectively) and that it includes shorter papers, comments, and articles in symposia and special addresses (but still not replies and errata). One departure from Siegfried is that I always assign authors to their first affiliation rather than splitting credit for authors who list affiliations with two or more schools.

<sup>38</sup>Most of the increase between the 1980's and 1990's is attributable to the top three schools' share of the *QJE* having increased from 15.7 percent to 32.2 percent. The increase from the 1970's to the 1980's, however, is in a period where the top eight schools' share of the *QJE* was declining, and there is still an increase between the 1980's and 1990's if one removes the *QJE* from the calculation.

<sup>39</sup>I measure Chicago's combined share of the three journals in the 1990's as 6.0 percent compared to 6.4 percent reported by Siegfried (1994) for the 1970's. Chicago's share of *QJE* pages was 1.1 percent in the 1970's and 8.8 percent in the 1990's.

<sup>40</sup>I assigned gender and native English dummies to all first names (or middle names following an initial) that appeared in the data. Authors who gave only initials are dropped from the numerator and denominator. This process doubtless produced a number of errors, so I would be hesitant to regard the levels (as opposed

total author pool the changes are small.

My conclusion from Table 5 is that it is hard to find much evidence of a democratization of the review process in the composition of the pool of published papers.

#### 4.2.2 Evidence from cross-sectional variation

I now turn to the paper-level data. To examine whether there has been a democratization the obvious thing to do with this data is to look for evidence that high status authors were favored in the earlier years. The most relevant question that I can address here is whether papers by high status authors that were accepted made it through the review process more quickly.<sup>41</sup> I discuss a number of variables that may (among other things) proxy for high status: publications in *Brookings Papers on Economic Activity* and the *AER's Papers and Proceedings* issue, publications in earlier decades, institutional affiliation and current decade research productivity.

Before discussing the results, I will take some time to provide more detail on set up that is common to all of the regression results I'll discuss in the paper. As mentioned above, I have obtained data on submit-accept times for most papers published in *Econometrica*, *REStud* and *REStat*, papers published in the *JPE* and *AER* since 1992 or 1993, and papers published in the *QJE* in 1973-1977, 1980, 1985, 1990, and since 1993. The data end at the end of 1997 or the middle of 1998 for all journals. I will estimate separate regressions for each decade. I include papers in *REStat* in the 1970's sample, but not in subsequent decades. Estimates should be regarded as derived from a large subset of the papers in the 1990's and from smaller and less representative subsamples in the 1970's and (especially) the 1980's. The sample includes only standard full-length articles omitting (when feasible) shorter papers, comments, replies, errata, articles in symposia or special issues, addresses, etc.

Summary statistics on the sets of papers for which data is available in each decade are presented in Table 6. The summary statistics for the journal dummy variables provide a more complete view of what is in the sample in each decade. I have omitted summary statistics on the dummy variables that classify papers into fields. More information on how this was done is given in Section 5.2.2. A decade-by-decade breakdown of the fraction of papers in the top five journals which are classified as belonging to each field is given in to the trends) as meaningful.

<sup>41</sup>The question one might most like to ask is whether papers by high status authors are more likely to be accepted holding paper quality fixed. This, however, is not possible — I know little or nothing about the pool of rejected papers at top journals.

Appendix B.<sup>42</sup> I will not give the definitions of all of the variables here, but will instead discuss them in connection with the relevant results.

Table 6: Summary statistics for submit-accept time regressions

Variable	Sample					
	1970's		1980's		1990's	
	Mean	SD	Mean	SD	Mean	SD
<i>Lag</i>	300.14	220.27	498.03	273.84	659.55	360.90
<i>AuBrookP</i>	0.07	0.45	0.06	0.30	0.09	0.35
<i>AuP&amp;P</i>	0.19	0.49	0.27	0.70	0.36	0.78
<i>AuTop5Pubs70s</i>	2.20	2.51	1.02	1.77	0.43	1.34
<i>SchoolTop5Pubs</i>	—	—	—	—	35.47	32.07
<i>AuTop5Pubs</i>	2.20	2.51	2.55	1.87	1.89	1.36
<i>EnglishName</i>	0.65	0.45	0.67	0.43	0.66	0.41
<i>Female</i>	0.03	0.16	0.04	0.17	0.07	0.22
<i>UnknownName</i>	0.09	0.29	0.02	0.15	0.01	0.11
<i>JournalHQ</i>	—	—	—	—	0.08	0.27
<i>NumAuthor</i>	1.39	0.60	1.49	0.64	1.73	0.71
<i>Pages</i>	13.09	6.37	17.43	7.52	24.20	8.72
<i>Order</i>	6.81	4.08	6.40	3.70	5.39	3.15
$\log(1 + \text{Cites})$	2.52	1.31	2.91	1.28	2.33	1.03
<i>EditorDistance</i>					0.81	0.25
<i>AER</i>	0.00	0.00	0.00	0.00	0.18	0.38
<i>Econometrica</i>	0.41	0.49	0.56	0.50	0.26	0.44
<i>JPE</i>	0.00	0.00	0.00	0.00	0.18	0.38
<i>QJE</i>	0.09	0.28	0.06	0.23	0.18	0.38
<i>REStud</i>	0.24	0.43	0.38	0.49	0.20	0.40
<i>REStat</i>	0.26	0.44	0.00	0.00	0.00	0.00
Number of obs.	1564		1154		1413	
Sample coverage	51%		44%		74%	

The table reports summary statistics for the 1970's, 1980's and 1990's regression samples.

The dependent variable for the regressions, *Lag*, is the length of time in days between the submission of a paper and its final acceptance (or a proxy for this).<sup>43</sup> I use a number of variables to look for evidence that papers by high status authors are accepted more

<sup>42</sup>The means reported in the table in Appendix B differ from the means of the field dummies in the regression samples because they are computed for all full-length articles in the top five journals regardless of whether some data was unavailable and because they do not include data from *REStat*.

<sup>43</sup>Because of data limitations I substitute the length of time between the submission date and the date of final resubmission for papers in *Econometrica* and for pre-1975 papers in *REStud*. The 1973-1977 *QJE* data use the time between submission and a paper receiving its initial acceptance (which was not infrequently followed by a later resubmission).

quickly. The first two, *AuBrookP* and *AuP&P* are average number of papers that the authors published in *Brookings Papers on Economic Activity* and the *AER's Papers and Proceedings* issue in the decade in question.<sup>44</sup> Papers published in these two journals are invited rather than submitted, making them a potential indicator of authors who are well known or well connected.<sup>45</sup> Estimates of the relationship between publication in these journals and submit-accept times during the 1970's can be found in column 1 of Table 7. The estimated coefficients on *AuBrookP* and *AuP&P* are statistically insignificant and of opposite signs. They provide little evidence that "high status" authors enjoy substantially faster submit-accept times. The results for the 1980's and 1990's in the other two columns are qualitatively similar. The estimated coefficients are always insignificant and the point estimates on the two variables have opposite signs.

A second idea for constructing a measure of status is to use publications in an earlier period. Unfortunately, I am limited by the fact that my database of publications (obtained from *Econlit*) only starts in 1969. I am able to include publications in the top five journals in the 1970's, *AuTop5Pubs70s*, as a potential indicator of high status in the 1980's and 1990's regressions.<sup>46</sup> The coefficient estimates for this variable in the two decades, reported in columns 2 and 3 of Table 7, are very small and neither is statistically significant.

A third potential indicator of status is the ranking of the institution with which an author is affiliated. Here I am even more limited in analyzing the "old days" in that my data do not start until the 1990's. I do include in the 1990's regression a variable, *SchoolTop5Pubs*, giving the total number of articles by authors at the author's institution in the 1990's.<sup>47</sup> The distribution of publications by school is quite skewed. The measure

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<sup>44</sup>More precisely author-level variables are defined first by taking simple counts (not adjusted for coauthorship) of publications in the two journals. Article-level variables are then defined by taking the average across the authors of the paper. Here and elsewhere throughout the paper I lack data on all but the first author of papers with four or more authors.

<sup>45</sup>To give some feel for the variable, the top four authors in *Brookings* in 1990 - 1997 are Jeffrey Sachs, Rudiger Dornbush, Andrei Shleifer and Robert Vishny, and the top four authors in *Papers and Proceedings* in 1990 - 1997 are James Poterba, Kevin Murphy, James Heckman and David Cutler. Another justification for the status interpretation is that both *AuBrookP* and *AuP&P* are predictive of citations for papers in my dataset.

<sup>46</sup>This variable is defined using my standard set of five journals, giving fractional credit for coauthored papers, and omitting short papers, comments, papers in symposia, etc. The variable is first defined each authors and I create a paper-level variable by averaging these values across the coauthors of a paper.

<sup>47</sup>This variable is defined using my standard set of five journals, giving fractional credit for coauthored papers, and omitting short papers, comments, papers in symposia, etc. The variable is first defined each authors and I create a paper-level variable by averaging these values across the coauthors of a paper. Each author is regarded as having only a single affiliation for each paper, which I usually take to be the first affiliation listed (ignoring things like "and NBER", but also sometimes names of universities that may represent an author's home or the institution he or she is visiting). Many distinct affiliations were manually combined to avoid splitting up departments from local research centers, and to correct misspellings and

Table 7: Basic submit-accept time regressions

Variable	Sample					
	1970's		1980's		1990's	
	Coef.	T-stat.	Coef.	T-stat.	Coef.	T-stat.
<i>AuBrookP</i>	15.4	1.15	-27.7	0.90	-26.2	1.24
<i>AuP&amp;P</i>	-5.0	0.39	16.2	1.09	16.9	1.33
<i>AuTop5Pubs70s</i>	—	—	1.5	0.30	4.1	0.59
<i>SchoolTop5Pubs</i>	—	—	—	—	-0.3	0.92
<i>AuTop5Pubs</i>	-6.9	2.54	-2.3	0.46	-16.3	2.15
<i>EnglishName</i>	1.3	0.09	4.3	0.22	-2.4	0.11
<i>Female</i>	-37.3	1.10	-56.9	1.25	49.0	1.11
<i>UnknownName</i>	3.1	0.14	-9.8	0.18	-5.2	0.06
<i>JournalHQ</i>	—	—	—	—	7.9	0.22
<i>NumAuthor</i>	-21.8	2.38	16.1	1.23	23.1	1.68
<i>Pages</i>	5.5	5.55	5.0	3.93	5.4	4.35
<i>Order</i>	1.8	1.29	4.9	2.06	8.6	2.69
<i>log(1 + Cites)</i>	-21.4	4.83	-11.8	1.65	-38.8	3.67
Journal Dummies	Yes		Yes		Yes	
Journal Trends	Yes		Yes		Yes	
Field Dummies	Yes		Yes		Yes	
Number of obs.	1564		1154		1413	
R-squared	0.12		0.10		0.19	

The table presents estimates from three regressions. The dependent variable for each regression, *Lag*, is the length of time between the submission of a paper to a journal and its acceptance in days (or a proxy). The samples are subsets of the set of papers published in the top five or six general interest economics journals between 1970 and 1998 as described in the text and in Table 6. The regression is estimated separately for each decade. The independent variables are characteristics of the author(s) and the paper. All regressions include journal dummies, journal-specific linear time trends, and dummies for seventeen fields of economics. Coefficient estimates are presented along with the absolute value of the t-statistics for the estimates.



is about 100 for each of the top three schools, but only five other institutions have values above 35 and only fourteen have values between 20 and 35. The fact that economists at the top schools have a substantial share of all publications, however, results in the mean of *SchoolTop5Pubs* being 35.5. While we are all aware that the most “highly ranked” departments are not always the most productive, productivity does look to be very highly correlated with prestige in my data.<sup>48</sup> The estimated coefficient on *SchoolTop5Pubs* in column 3 of Table 7 indicates that authors from schools with higher output had their papers accepted slightly more quickly, but that the differences are not significant. The coefficient estimate of -0.32 is relatively small — such an effect would allow economists at the top schools to get their papers accepted about one month faster than economists from the bottom schools.<sup>49</sup> While this can not tell us about whether a position at a top school conferred a status advantage in the 1970’s it does confirm that the compositional argument that mean times might be longer now because the pool of published papers has shifted to include more economists from lower ranked schools is not important.<sup>50</sup>

I have also included one additional variable in the regressions, *AuTop5Pubs*; that may proxy for status, but which is more difficult to interpret. The variable is the average number of articles that a paper’s authors published in top five journals in the decade in question.<sup>51</sup> Authors who are publishing more in top journals may be regarded as having high status. Any negative relationship between *AuTop5Pubs* and *Lag* may also be given an endogeneity interpretation. The authors who are able to publish a lot of papers in top journals will disproportionately be those who (whether by luck, hard work, or ability) are very efficient at getting their papers through the journals and thus have the time to write more papers. The regression results provide fairly clear evidence that authors who are more successful in a decade in getting their papers in the top journals are also getting their papers accepted more quickly. The estimates on *AuTop5Pubs* are negative in all three decades, and the 1970’s and 1990’s estimates are highly significant. While the estimated coefficient for the 1990’s is about two and a half times as large as the estimated coefficient for the

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variations in how names are reported, but this is a difficult task and some errors surely remain, especially at foreign institutions. Different academic units within the same university are also combined.

<sup>48</sup>For example, the top ten schools in the 1990’s according to the measure are Harvard, MIT, Chicago, Northwestern, Princeton, Stanford, Pennsylvania, Yale, UC-Berkeley and UCLA. The second ten are Columbia, UCSD, Michigan, Rochester, the Federal Reserve Board, Boston U, NYU, Tel Aviv, Toronto and the London School of Economics.

<sup>49</sup>To the extent that there is a relationship between the school productivity variable and submit-accept times it looks very linear.

<sup>50</sup>Of course, we already knew this because we saw that there has not been a shift in the pool of accepted papers in the direction of including more economists from outside the top schools.

<sup>51</sup>Again, fractional credit is given for coauthored papers.

1970's, given that mean submit-accept times are more than twice as long in the 1990's as in the 1970's the results can be thought of as indicating that this effect is of roughly the same magnitude throughout the period. The constancy of the effect does make me feel comfortable in concluding that if the results are indicative of a status benefit, they are reflecting a benefit which has not declined over time. I take a general theme from these regressions to be that it is hard to find any evidence of a democratization in looking at which authors had faster submit-accept times in the 1970's, 1980's and 1990's.

A second motivation for looking at the cross-sectional pattern of submit-accept times (in addition to simply asking whether there was a democratization) is to examine the arguments for why mean submit-accept times might have increased if there was a democratization. I have already addressed two. First, I found no evidence that mean submit-accept times have increased because high status authors used to enjoy prestige benefits and now do not. Second, the fact that there is not much of a relationship between submit-accept times and school rankings is inconsistent with the idea that mean review times might have increased because more authors are from lower ranked schools and get less help from their colleagues. To test one other potential explanation I included in the regressions a variable for whether the authors of a paper have first names suggesting that they are native English speakers, *EnglishName*.<sup>52</sup> Estimated coefficients on this variable are extremely small and highly insignificant in each decade. I already mentioned that the increase in the number of non-native English speakers publishing in the top journals has been slight. Together these results clearly indicate that the idea that more authors today may be non-native English speakers who need more editorial help is not relevant to understanding the slowdown.

## 5 Complexity and specialization

This section examines a set of explanations for the slowdown of the economics publishing process based on the common perception that economics papers are becoming more complex and the field more specialized. In general, I find little evidence that the profession has become more specialized and also find few of the links necessary to make increased complexity a candidate explanation for the slowdown. One connection I do find is that economics papers are becoming longer and longer papers have longer submit-accept times in the cross-section. This relationship might account for one to two months of the slowdown.

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<sup>52</sup>Here again I take an average of the authors' characteristics for coauthored papers. Switching to an indicator equal to one if any author has a name associated with being a native English speaker does not change the results.

## 5.1 The potential explanation

It seems to be a fairly common belief that economics papers have become increasingly technical, sophisticated and specialized over the last few decades. There are at least three reasons why such a trend could lead to a lengthening of the review process.

First, it may take longer for referees and editors to read and digest papers that are more complex.

Second, increased complexity and specialization may make it necessary for authors to get more input from referees. One story would be that increased complexity reduces authors' understanding of their own papers, so that they need more help from referees and editors to get things right. A related story I find more compelling is that in the old days authors were able to get advice about expositional and other matters from colleagues. With increasing specialization colleagues are less able to provide this service, and it may be necessary to substitute advice from referees.

Third, increased complexity and specialization may lead editors to change the way they handle papers. In the old days, this story goes, editors were able to understand papers and digest referee reports, clearly articulate what improvements would make the paper publishable, and then check for themselves whether the improvements had been made on resubmission. Now, being less able to understand papers and referees' comments, editors may be less able to determine and describe *ex ante* what revisions would make a paper publishable, which leads to multiple rounds of revisions. In addition, as editors lose the ability to assess revisions, more rounds must be sent back to referees, lengthening the time required for each round.

## 5.2 Has economics become more complex and specialized?

Let me first suggest that for a couple of reasons we should not regard it as obvious that economics has become more complex over the last three decades. First, by 1970 there was already a large amount of very technical and inaccessible work being done, and the 1990's has seen the growth of a number of branches with relatively standardized easy-to-read papers, e.g. natural experiments, growth regressions, and experimental economics. To take one not so random sample of economists, the Clark Medal winners of the 1980's were Michael Spence, James Heckman, Jerry Hausman, Sandy Grossman and David Kreps, while the 1990's winners were Paul Krugman, Lawrence Summers, David Card, Kevin Murphy and Andrei Shleifer.

Second, what matters for the explanations above is not that economics papers are more

complex, but rather that they are more difficult for economists (be they authors, referees or editors) to read, write and evaluate. While the game theory found in current industrial organization theory papers might be daunting to an economist transported here from the 1970's, it is second nature to researchers in the field today. In its February 1975 issue, the *QJE* published articles by Joan Robinson and Steve Ross. The August issue included papers by Nicholas Kaldor and Don Brown. To me, the range of skills necessary to evaluate these papers seems much greater than that necessary to evaluate papers in a current *QJE* issue.

### 5.2.1 Some simple measures

In a couple of easily quantifiable dimensions, papers have changed in a manner consistent with increasing complexity.

Figure 4 graphs the median page length of articles over time at the top general interest journals.<sup>53</sup> As noted by Laband and Wells (1998) there has been a fairly rapid growth in the length of published papers since 1970. At the *AER*, *JPE* and *QJE* articles are now about twice as long as they were in 1970. At *Econometrica* and *REStud* articles are about 75% longer. Only *REStat* shows a more modest growth.

A second trend in economics publishing that has been noted elsewhere is an increase in coauthorship (Hudson, 1996). In the 1970's only 30 percent of the articles in the top five journals were coauthored. In the 1990's about 60 percent were coauthored. In the longer run the trend is even more striking: as recently as 1959 only 3 percent of the articles in the *Journal of Political Economy* were coauthored. This trend could be indicative of an increase in complexity if one reason that economists work jointly on a project is that one person alone would find it difficult to carry out the range of specialized tasks involved.

While each of these changes could be indicative of an increase in complexity, other interpretations are possible. One potential problem with the page length measure is that it may reflect also the degree to which journals require authors to provide a detailed introduction, give intuition for equations, survey related literatures, and do other things that are intended to make papers easier to read rather than harder. Laband and Wells (1998) note that prior to 1970 there had been a gradual but substantial trend toward shorter papers dating all the

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<sup>53</sup>To be precise, the figure and the discussion in this paragraph concerns the median of the page lengths of articles which were among the first five in their issue. This measure was chosen to reflect the length of a "typical" article in a way that would be unaffected by changes over time in the number of notes that are published and in changing definitions of what constitutes a paper versus a note. I do not attempt to correct for slight format changes instituted at the *JPE* in 1971 and at *REStud* in 1982 because my attempts to count typical numbers of characters per page indicated that there were no substantial changes.

Median Article Lengths: 1969 - 1999

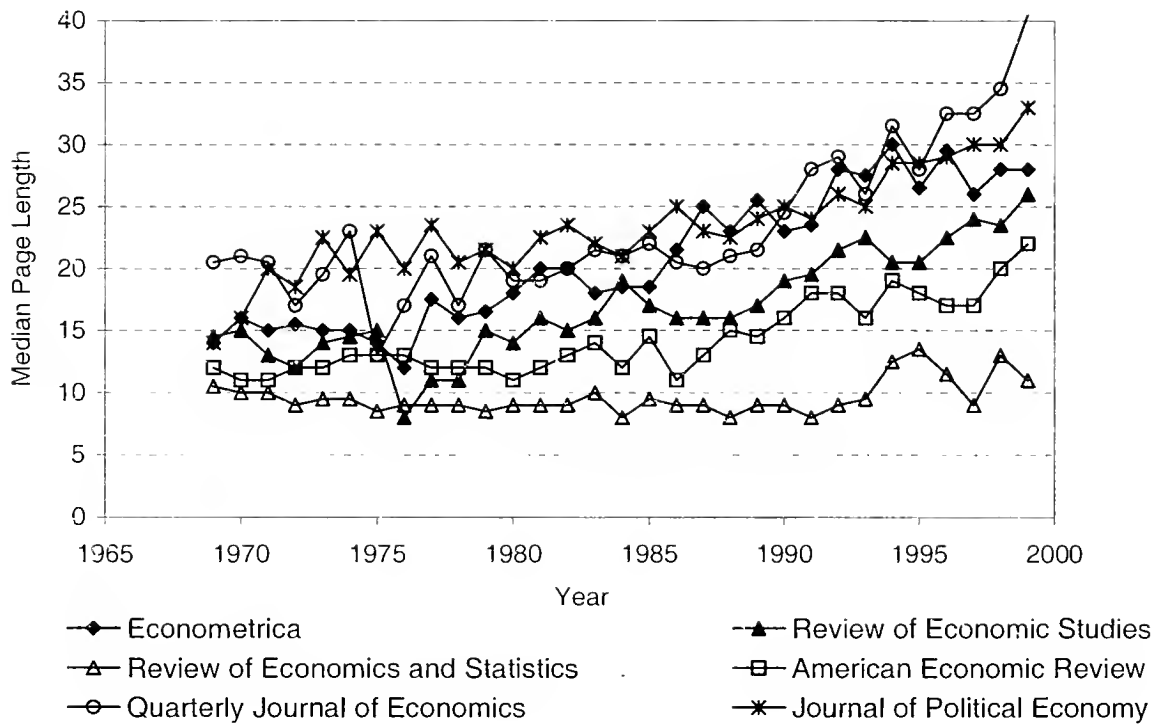


Figure 4: Changes in page lengths over time

The figure graphs the median length in pages of articles that were among the first five articles in their journal issue.

way back to the turn of the century. Hence, a second problem is that if one wants to regard complexity as continually increasing, then one must argue that page lengths switched from being negatively related to complexity to positively related in 1970. A troubling fact about coauthorship as a measure of complexity is that in recent years coauthorship has been less common at the *Review of Economic Studies* and *Econometrica* than at the *AER*, *QJE* and *JPE*.<sup>54</sup>

One additional (albeit somewhat circular) piece of evidence on complexity is the first review times we saw earlier. Recall from Figure 3 that there has been only a small increase in journals' first response times over the last fifteen or twenty years. If papers were now more difficult to read, one might expect these times to have increased.<sup>55</sup> The widening gap between first response times for all papers and for eventually accepted papers at the *JPE* may also be informative. It seems more likely that this reflects referees and editors spending longer developing ideas for more substantial revisions than that there is a widening gap between the complexity of accepted and rejected papers.

### 5.2.2 Measures of specialization

As noted above, the relevant notion of complexity for the stories told above is complexity relative to the skills and knowledge of those in the profession. In this subsection, I look for evidence of complexity in this sense, by examining the extent to which economists have become more or less specialized over time. My motivation for doing so is the thought that if there has been an increase in complexity that has made it more difficult for authors to master their own work, for colleagues to provide useful feedback, and/or for editors to digest papers, then economists should have responded by becoming increasingly specialized in particular lines of research. I find little evidence of increasing specialization.

To measure the degree to which economists are specialized I use the index that Ellison and Glaeser (1997) proposed to measure geographic concentration.<sup>56</sup> Suppose that a set of

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<sup>54</sup>The most obvious alternative to increasing complexity as the cause of increasing coauthorship is changes in the returns to writing coauthored papers. Sauer's (1988) analysis of the salaries of economics professors at seven economics departments in 1982 did not support the common perception that the benefit an economist receives from writing an  $n$ -authored is greater than  $1/n^{th}$  of the benefit from writing a sole authored paper.

<sup>55</sup>As mentioned above it is not clear how closely review times and difficulty of reading should be linked given that the time necessary to complete a review is a tiny fraction of the time referees hold papers. Another possibility is that referees might respond to the increased complexity of submissions by reading papers less carefully. This could also account for a trend toward more rounds of revisions, but I know of no evidence to suggest that it is true.

<sup>56</sup>The analogy with Ellison and Glaeser (1997) is to equate economists with industries, fields with geographic areas, and papers with manufacturing plants. See Stern and Trajtenberg (1998) for an application of the index to doctors' prescribing patterns similar to that given here.

economics papers can be classified as belonging to one of  $F$  fields indexed by  $f = 1, 2, \dots, F$ . Write  $N_i$  for the number of papers written by economist  $i$ ,  $s_{if}$  for the share of economist  $i$ 's papers that are in field  $f$ , and  $x_f$  for the fraction of all publications that are in field  $f$ . The Ellison-Glaeser index of the degree to which economist  $i$  is specialized is

$$\gamma_i = -\frac{1}{N_i - 1} + \frac{N_i}{N_i - 1} \sum_f (s_{if} - x_f)^2 / (1 - \sum_f x_f^2).$$

Under particular assumptions discussed in Ellison and Glaeser (1997) the expected value of this index is unaffected by the number of papers by an author that we are able to observe, and by the number and size of the fields used in the breakdown. The scale of the index is such that a value of 0.2 would indicate that the frequency with which we see pairs of papers by the same author being in the same field matches what would be expected if 20 percent of authors wrote all of their papers in a single field and 80 percent of authors wrote in fields that were completely uncorrelated from paper to paper (drawing each topic from the aggregate distribution of fields.)

I first apply the measure to look at the specialization of authors across the main fields of economics. Based largely on JEL codes, I assigned the articles in the top five journals since 1970 to one of seventeen fields.<sup>57</sup> In order of frequency the fields are: microeconomic theory, macroeconomics, econometrics, industrial organization, labor, international, public finance, finance, development, other, urban, history, experimental, productivity, political economy, environmental, and law and economics.

Table 8 reports the average value of the Ellison-Glaeser index (computed separately for the 1970's, 1980's and 1990's) among economists having at least two publications in the top five journals in the decade in question.<sup>58</sup> The data in the first three columns indicate that there has been only a very slight increase in specialization. The absolute level of specialization also seems fairly low relative to the common perception.

The most obvious bias in the construction of this series is that with the advent of the new JEL codes in 1991 I am able to do a better job of classifying papers into fields.<sup>59</sup> Misclassifications will tend to make authors' publishing patterns look more random and decrease measured specialization. Hence, the calculations above may be biased toward finding

<sup>57</sup>In a number of cases the JEL codes contain sets of papers that seem to belong to different fields. In these cases I used rules based on title keywords and in some cases paper-by-paper judgements to assign fields.

<sup>58</sup>I take an unweighted average across economists, so the measure reflects the specialization of the large number of economists who have a few top publications and gives less weight to people like Joseph Stiglitz and Martin Feldstein than their share of publications would dictate.

<sup>59</sup>A related bias is that it may be easier for me to divide papers into fields in the 1990's because my understanding of what constitutes a field is based on my knowledge of economics in the 1990's.

Table 8: Specialization of authors across fields over time

	Decade		
	1970's	1980's	1990's
Mean EG index	0.33	0.33	0.37

The table reports the mean value of the Ellison-Glaeser concentration index computed from the decade-specific top five journal publication histories of authors with at least two papers in the sample in the decade in question. Seventeen fields are used for the analysis. Data for the 1990's includes data up to the end of 1997 or mid-1998 depending on the journal.

increased specialization. To assess the potential magnitude of this bias, I recomputed the specialization index for the 1990's after reclassifying the 1990's papers using only the old JEL codes (and the same rules I had used for the earlier papers.) When I did this, the measure of specialization in the 1990's declines to 0.31, a value which is below the level for the 1970's and 1980's. I conclude that there is very little if any evidence of a trend toward increasing specialization across fields.

The results above concern specialization at the level of broad fields. A second relevant sense in which economists may be specialized is within particular subfields of the fields in which they work. To construct indices of within-field specialization, I viewed each field of economics (in each decade) as a separate universe, and treated pre-1991 JEL codes as subfields into which the field could be divided. I then computed Ellison-Glaeser indices exactly as above on the set of economists having two or more publications in top five journals in the field (ignoring their publications in other fields). In the minor fields this would have left me with a very small (and sometimes nonexistent) sample of economists. Hence, I restricted the analysis to the seven fields for which the relevant sample of economists exceeded ten in each decade and for which the subfields defined by JEL codes gave a reasonably fine field breakdown: microeconomic theory, macroeconomics, labor, industrial organization, international, public finance and finance.<sup>60</sup>

The results presented in Table 9 reveal no single typical pattern. In three fields, microeconomic theory, industrial organization and labor, there is a trend toward decreasing within-field specialization. In two others, macroeconomics and public finance, there is a substantial drop from the 1970's to the 1980's followed by a slight increase from the 1980's to the 1990's. International economics and finance, in contrast, exhibit increasing within-field

<sup>60</sup>The number of economists meeting the criterion ranged from 19 for finance in the 1970's to 264 for theory in the 1980's. The additional restriction was that I only included fields for which the herfindahl index of the component JEL codes was below 0.5.



specialization.

Table 9: Within-field specialization of authors over time

Field	Index of within-field specialization		
	1970's	1980's	1990's
Microeconomic theory	0.38	0.32	0.23
Macroeconomics	0.27	0.17	0.18
Industrial organization	0.35	0.30	0.11
Labor	0.27	0.22	0.09
International	0.25	0.35	0.36
Public Finance	0.50	0.28	0.30
Finance	0.29	0.20	0.41

The table reports the mean value of the Ellison-Glaeser concentration index computed by treating publications in a field in the top five journals in a decade as the universe and treating the set of distinct pre-1991 JEL codes of papers in the field as the set of subfields. Values are the unweighted means of the index across authors with at least two such publications. Data for the 1990's includes data up to the end of 1997 or mid-1998 depending on the journal.

Again, one potential bias in the time series is that I do a better job of classifying papers after 1990. Misclassifications of papers into fields will tend to make within-field specialization look higher. For example, if a JEL code containing a few macro papers is put into micro theory, a few macroeconomists will be added to the micro theory population. Their publications in the micro theory universe will tend to be concentrated in the misclassified JEL code. By improving the classification in the 1990's I may be biasing the results toward a finding of reduced within-field specialization. To assess this bias I again repeated the calculations after reclassifying the 1990's data using only the pre-1991 JEL codes. This change increased the measured within-field specialization for the 1990's for all fields except public finance. In no case, however, did the ranking of 1970's versus 1990's specialization change. The largest change is in theory, where the 1990's value of the specialization index, 0.36, becomes very close to its 1970's value.

A second potential bias is that the relevance of the subfields defined by JEL codes changes over time. In some cases, such as the creation of new JEL codes for auction theory and contract theory in 1982, the JEL codes themselves change in a way that make them better descriptions of subfields. This would tend to make measured specialization increase. In other cases, fields evolve in a way that causes the JEL codes to lose their ability to describe meaningful subfields. In empirical industrial organization, for example,

the codes mostly describe the industry being studied, rather than the topic that is being explored using the industry as an example or whether the author takes a reduced form or structural approach.<sup>61</sup> To get some idea of how this may affect the results, I constructed my own breakdown of microeconomic theory into ten subfields. In order of frequency they are: unclassified, price theory, general equilibrium, welfare economics, game theory, social choice, contract theory, auctions, decision theory and learning. The classification is largely made by combining JEL codes, but again I also in some cases use title keywords or case-by-case decisions. Using these subfields, I find the within-theory specialization index for the three decades to be 0.40, 0.28 and 0.45. (Here, the fact that my subfield classifications improve over time may bias me toward finding increased specialization.)

Overall, I interpret the results of this section as indicating that there is little evidence of a trend toward economists becoming more specialized.

### 5.2.3 Why might economists perceive that specialization has increased?

How can we reconcile the results of the previous section with a common perception that economics is becoming increasingly specialized? One set of potential explanations is based on the fact that economists and their positions within the profession change over time, and judgements about changes in complexity are biased by changes in one's perspective. One potential effect is that economists may invest heavily in knowledge capital at the start of their career and then allow their knowledge to decay over time. They would then correctly perceive themselves to understand less of the field over time, regardless of whether the understanding of the profession as a whole has changed. Another source of bias may be that what economists are asked to do changes over time. Initially, economists are only asked to referee papers closely related to their work. Later, they are put in roles where they read papers further from their specialty, e.g. reviewing colleagues for tenure and serving on hiring committees. If they don't fully account for changes in the set of papers they read, economists may perceive their ability to read papers to have diminished. Another factor could be changing expectations that make economists more uncomfortable with a lack of knowledge as they advance to higher positions. If economists form beliefs about how complexity has changed by thinking of their recent observations of old papers another bias is plausible. The old papers that economists encounter are a nonrandom sample of the papers written at the time. They tend to be papers that have spawned substantial future

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<sup>61</sup>Another example is that a primary breakdown of microeconomic theory in the old codes is into consumer theory and producer theory.

work. Such papers will be easier to understand today than when they were written.

### 5.3 Links between complexity and review times

In this section I'll put aside the question of whether economics papers really are becoming more complex and discuss a few pieces of evidence on the question of whether an increase in complexity would slow down the review process if it were occurring.

#### 5.3.1 Simple measures of complexity

I noted earlier that papers have grown longer over time and that coauthorship is more frequent. While it is not clear whether these changes are due to an increase in the complexity of economics articles, it is instructive to examine their relationship with submit-accept times. Two variables in the regression of submit-accept times on paper and author characteristics in Table 7 are relevant.

First, *Pages*, is the length of an article in pages.<sup>62</sup> In all three decades, this variable has a positive and highly significant effect.<sup>63</sup> The estimates are that longer papers take longer in the review process by about five days per page. The lengthening of papers over the last thirty years might therefore account for two months of the overall increase in submit-accept times. Alternate explanations for the estimate can also be given. For example, papers that go through more rounds of revisions may grow in length as authors add material and comments in response to referees' comments, or longer published papers may tend to be papers that were much too long when first submitted and needed extensive editorial input. It is also not clear whether increases in page lengths should be regarded as a root cause or whether they are themselves a reflection of changes in social norms for how papers should be written.

Second, *NumAuthors* is the number of authors of the paper. In the 1970's, coauthored papers appear to have been accepted more quickly. In later decades coauthored papers have taken slightly longer in the review process, but the relationship is not significant. I would conclude that if the rise in coauthorship is due to the increased difficulty of writing an economics papers, then in the cross-section any tendency of coauthored papers to be more complex and take longer to review must be largely offset by advantages to the authors of having multiple authors working on the paper.

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<sup>62</sup>Recall that the regression includes only full-length articles and not shorter papers, comments and replies.

<sup>63</sup>This contrasts with Laband et al (1990) who report that in a quadratic specification the relationship between review times and page lengths (for papers in *REStat* between 1970 and 1980) is nearly flat around the mean page length. Hamermesh (1994) does report that referees take longer to referee longer papers in his data, but the size of that effect (about 0.7 days per page) is too small to fully account for what I observe.

### 5.3.2 Specialization and advice from colleagues

In this section I focus on the second potential link between complexity and review times mentioned above — that in an increasingly specialized profession authors will be less able to get help from their colleagues. The data provides little support for this idea.

The argument above is based on an assumption that advice from colleagues is useful and gives authors a headstart on the journal review process. If this were true, economists from top departments should get their papers through the review process more quickly than economists at departments which produce less research output. Economists at top schools are more likely to have colleagues with sufficient expertise in their area to provide useful feedback than are economists in smaller departments or in departments where fewer of the faculty are actively engaged in research.

Recall that I had earlier included the variable *SchoolTop5Pubs* in my basic regression of submit-accept times on author and editor characteristics in the hope that it might reveal a prestige advantage enjoyed by authors at top schools. The variable would also be expected to have a negative sign if these authors enjoyed real advantages in the form of helpful advice from colleagues. The fact that the t-statistic on the variable (in the third column of Table 7) is only 0.9 indicates that I do not find significant evidence that interacting with more productive colleagues allows one to polish papers prior to submission and thereby reduce submit-accept times.<sup>64</sup>

### 5.3.3 Specialization and editor expertise

In this subsection, I examine the argument that submit-accept times may lengthen as the profession becomes more specialized because an editor with less expertise on a topic will end up asking for more rounds of revisions and sending more revisions back to the referees. This argument is certainly plausible, but the opposite effect would be plausible as well. Indeed, one editor remarked to me a few years ago that he felt that the review process for the occasional international trade paper that he handled was less drawn out than for papers in his specialty. The reason was that for papers in his specialty he would always

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<sup>64</sup>While the regression provides no evidence of a relationship between affiliation on submit-accept times as hypothesized above, it is interesting to note that authors from top schools do get their papers accepted more quickly. In a univariate regression of submit-accept times on *SchoolTop5Pubs*, the coefficient estimate is -1.09 with a t-statistic of 3.66. Whether one regards this as indicating that the structure of the profession puts economists at lower ranked schools at a disadvantage will depend on one's view of the *QJE*. The measured effect drops in half when journal dummies and journal-specific time trends are included, and it becomes insignificant when the other control variables (most notably the author's publication record) are included. The primary reason for this is that the *QJE* has the fastest submit-accept times and the fraction of papers coming from top schools is substantially higher there than at the other journals.

identify a number of ways in which the paper could be improved, while with trade papers if the referees didn't have many comments he would just have to make a yes/no decision and focus on the exposition.

My idea for examining the editor-expertise link between specialization and submit-accept times is straightforward. I construct a measurement, *EditorDistance*, of how far the paper is from the editor's area of expertise and include this in a submit-accept time regression like those in Table 7.

The approach I take to quantifying how far each paper is from its editor's area of expertise is to assign each paper  $i$  to a field  $f(i)$ , determine for each editor  $e$  the fraction of his papers,  $s_{eg}$ , falling into each field  $g$ , define a field-to-field distance measure,  $d(f, g)$ , and then define the distance between the paper and the editor's area of expertise by

$$EditorDistance_i = \sum_g s_{e(i)g} d(f(i), g).$$

When the editor's identity is not known, I evaluate this measure for each of the editors who worked at the journal when the paper was submitted and then impute that the paper was assigned to the editor for whom the distance would be minimized.<sup>65</sup>

The construction of the field-to-field distance measure is based on the idea that two fields can be regarded as close together if economists who write papers in one are also likely to write in the other. Details on how this was done are reported in Appendix A. The whole exercise may seem a bit far fetched, so I have also included a couple of tables in the appendix designed to give an idea of how the measure is working: one lists the three closest fields to each field; the other presents some examples of imputed editor assignments and distances. I'd urge anyone interested to take a look.

Table 10 reports the estimated coefficient on *EditorDistance* in regressions of submit-accept times in the 1990's on this variable and the variables in the basic regression of Table 7. To save space, I do not report the coefficient estimates for the other variables, which are similar to those in Table 7.<sup>66</sup> The specification in the first column departs slightly from the earlier regressions in that it employs editor fixed effects rather than journal fixed effects and journal-specific trends. The coefficient estimate of -66.8 indicates that papers that are further from the editor's area of expertise had slightly *shorter* submit-accept times (the standard deviation of *EditorDistance* is 0.25), but the effect is not statistically significant.

<sup>65</sup>The data include the editor's identity only for papers at the *JPE* and papers at the *QJE* in later years. All other editor identities are imputed.

<sup>66</sup>The most notable change is in the coefficient on  $\log(1 + Cites)$  increases to 65.9 and its t-statistic increases to 6.65 while the coefficient on *Order* becomes smaller and insignificant. The interpretation of these variables will be discussed in Section 7.2.2.

Table 10: Effect of editor expertise on submit-accept times

Independent Variables	Dependent variable: submit-accept time		
	(1)	(2)	(3)
<i>EditorDistance</i>	-66.8 (1.3)	-146.9 (3.4)	-22.4 (0.5)
Editor fixed effects	Yes	Yes	No
Field fixed effects	Yes	No	Yes
Journal fixed effects and trends	No	No	Yes
Other variables from Table 7	Yes	Yes	Yes
R-squared	0.30	0.27	0.19

The table reports the results of regressions of submit-accept times on the distance of a paper from the editor's area of expertise. The sample consists of papers published in the top five general interest journals in the 1990's for which the data is available. The dependent variable is the time between a papers submission to the journal and its acceptance (or final resubmission in the case of *Econometrica*) in days. The primary independent variable, *EditorDistance* is a measure of how far the paper is from the editor's area of expertise as described in the text and Appendix A. T-statistics are given in parentheses below the estimates. The regression in column (1) has unreported editor and field fixed effects. The regression in column (2) has editor fixed effects. The regression in column (3) has field and journal fixed effects and field specific linear time trends. Each regression also includes the same independent variables as in the regressions in Table 7.

The regression in the first column includes both editor and field fixed effects (for seventeen fields). In each case, one might argue that including the fixed effects ignores potentially interesting sources of variation. First, some fields have been much better represented than others on the editorial boards of top journals. For example, the *AER*, *QJE*, *JPE* and *Econometrica* have all had labor economists on their boards for a substantial part of the last decade, while I don't think that any editor (of forty two) would call himself an international economist.<sup>67</sup> One could imagine that this might lead to informative differences in the mean submit-accept times for labor and international papers that are ignored by the field fixed-effects estimates. Column 2 of Table 10 reports estimates from a regression which is like that of column 1, but omitting the field fixed effects. The coefficient estimate for *EditorDistance* is now -146.9, and it is highly significant. Apparently, fields which are well represented on editorial boards have slower submit-accept times.<sup>68</sup>

Column 3 of Table 10 reports on a regression that omits the editor fixed effects (and includes journal fixed effects and journal specific linear time trends). The motivation for this specification is that if editor expertise speeds publication then the editors of a journal who handle fewer papers outside their area should on average be faster. The fact that the coefficient on *EditorDistance* is somewhat less negative in column 3 than in column 1 provides only very weak support for this hypothesis.<sup>69</sup>

Overall, I conclude that I have found little evidence of any mechanism by which increased specialization would lead to a slowdown of the review process.

## 6 Growth of the profession

In this section I discuss the idea that the slowdown may be a consequence of the growth of the economics profession. What I do and do not find about how the profession has changed may be surprising. First, what I do not find is evidence that the profession has grown much over the last thirty years or that many more papers are being submitted to top journals.

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<sup>67</sup>It is also true that none of the forty two are women. Nancy Stokey and Valerie Ramey did not start in time to have any papers published before the end of my data.

<sup>68</sup>A problem with trying to interpret this as indicating that economists in a field are made better or worse off by being represented on editorial boards is that I can say nothing about the effect of editor expertise on the likelihood of a paper of a particular quality being accepted. If such a relationship exists, the results on submit-accept times may also reflect a selection bias to the extent that the mean quality of papers in different fields differs.

<sup>69</sup>One potential problem with trying to use the cross-editor variation in expertise is an endogeneity issue — editors who handle a lot of papers outside their field may have gotten their jobs over editors who would have been a better match for the submissions fieldwise because it was thought that they would do a good job.

Hence, it does not appear that growth could have slowed the review process significantly by increasing editorial workloads. Second, what I do find is over the last two decades the top journals have grown substantially in their impact relative to other journals. Looking at patterns across journals I estimate that the increased competition that this creates may account for three months of the slowdown at the top journals.

## 6.1 The potential explanation

The starting point for the set of explanations I will discuss here is the assumption that there has been a great deal of growth in the economics profession over time. There are at least three main channels through which such growth might be expected to lead to a slowdown of the publication process.

First, an increase in the number of economists would be expected to lead to an increase in submissions, and thereby to increases in editors' workloads. Editors who are under time pressure may be more likely to return papers for an initial revision without having thought through what changes would make a paper publishable, and thereby increase the number of rounds of revisions that are eventually necessary. They may also rely more on referees to review revisions rather than trying to evaluate the changes themselves, which can lead both to more rounds and longer times per round.

Second, in the "old days" editors may have seen many papers before they were submitted to journals. With the growth of the profession, editors may have seen a much smaller fraction of papers prior to submission. Unfamiliar papers may have longer review times.

Third, growth would lead to more intense competition to publish in the top journals. This would be expected to lead to an increase in overall quality standards. To achieve the higher standards, authors may need to spend more time working with referees and editors to improve exposition, clarify proofs, address alternate explanations, etc.<sup>70</sup>

## 6.2 Has the profession grown?

While my first inclination was to not even ask this question assuming that the answer was obviously yes, evidence of substantial growth is hard to find.

First, recall from Table 5 that there has been little change since the 1970's in the Herfindahl index of the author-level concentration of publication, which suggests that the

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<sup>70</sup>Ellison (2000) provides an example where the opposite change occurs in an equilibrium model of time allocation. As the journal becomes more selective, authors gamble on increasingly bold ideas, and the polish of the average accepted paper declines.



population of economists trying to publish in top journals is not providing more severe competition for the top economists.

Second, as Siegfried (1998) has noted, counts of economists obtained from membership rolls of professional societies or department faculty lists also indicate that the profession has grown relatively slowly since 1970. Table 11 reports time series for the number of members of the American Economic Association and Econometric Society.<sup>71</sup> Increases in AEA membership since 1970 seem modest — the total increase over the last thirty years is about 10 percent. Siegfried (1998) also counted the number of economics department faculty members at 24 major U.S. universities at various points in time. In aggregate the economics departments at these universities were slightly smaller in 1995 than in 1973. The growth in the profession due to increases in the number of economists at business schools and other institutions is presumably a large part of the difference between the overall AEA membership increase and the slight drop in membership at the 24 economics departments he examined.

Econometric Society membership has increased more substantially since 1980.<sup>72</sup> The growth in individual memberships may overstate the growth in the number of economists interested in the *AER* and *Econometrica* for a couple reasons. At both journals some of the increase may be attributable to institutions switching subscriptions to individuals' names (the gap between individual and institutional prices has widened and the decrease in the institutional subscriber base is comparable to the increase in the individual total). The price of *Econometrica* has also declined over time in real terms.<sup>73</sup>

The number of U.S. members of the Econometric Society has only increased by about 10 percent between 1976 and 1998, so it may be tempting to try to reconcile the two series by hypothesizing that there has been relatively slow growth in the U.S. economist population, but substantial overall growth due to a more rapid growth in the number of economists outside the U.S. doing work that would be appropriate for top journals. This, however, is at odds with the publication data. The increase in authors with foreign names comes from

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<sup>71</sup>The table records the total membership of the AEA and the number of regular members of the Econometric Society at midyear.

<sup>72</sup>The earlier membership information is problematic. At the time the Econometric Society reported 1970 regular membership as 3150, which is above even the current total. However, the society at the time apparently had accounting problems that resulted in a large number of people continuing to remain members and receive the journal despite not having paid dues. The figure reported in the table for 1970 is an estimate obtained by adjusting the reported 1970 figure by the percentage drop in membership which occurred later when those who had not paid dues were dropped from the membership lists.

<sup>73</sup>I do not know of good estimates of price elasticities for journals, but the fact that subscriptions were so high in 1970 suggests that it could be large enough to allow most of the 13 percent increase in membership since 1990 to be attributed to the 20 percent cut in the real price over the period.

U.S. schools hiring foreign economists, not from the rise of foreign schools. In 1970 27.5 percent of the articles in the top five journals were by authors working outside the U.S.<sup>74</sup> In 1999 the figure was only 23.9 percent.<sup>75</sup>

Table 11: Growth in the number of economists in professional societies

	Year					
	1950	1960	1970	1980	1990	1998
AEA total membership	6936	10847	18908	19401	21578	20874
ES regular membership		1399	1955	1978	2571	2900

The first row of the total membership of the American Economic Association in selected years. The second row reports the number of regular members of the Econometric Society at midyear.

Finally, a third place where it seemed natural to look for evidence of the growth of the profession is in the number of submissions to top journals. Figure 5 graphs the annual number of new submissions to the *AER*, *Econometrica*, *JPE* and *QJE*. Generally the data indicate that there has been a small and nonsteady increase in submissions. *AER* submissions dropped between 1970 and 1980, grew substantially between 1980 and 1985, and have been fairly flat since (which is when the observed slowdown occurs). *JPE* submissions peaked in the early 1970's and have been remarkably constant since 1973.<sup>76</sup> *Econometrica* submissions grew substantially between the early 1970's and mid 1980's, and have generally declined since. *QJE* submissions increased at some point between the mid 1970's and early 1990's and have continued to increase in recent years. Overall, the submissions data indicate fairly clearly that there has not been a dramatic increase in submissions.

In both Table 11 and Figure 5 I have included some data from before 1970. The clarity of the evidence of the growth of the profession between 1950 and 1970 provides a striking contrast. American Economic Association membership grew by more than 50 percent in both the 1950's and the 1960's (and also more than doubled in the 1940's). The Econometric Society also appears to have grown substantially in the 1960's. Submissions to the *AER* grew from 197 in 1950 to 276 in 1960 and 879 in 1970. I take this data to suggest

<sup>74</sup>Each author of a jointly authored paper was given fractional credit in computing this figure, with credit for an author's contributions also being divided if he or she lists multiple affiliations (other than the NBER and similar organizations).

<sup>75</sup>The percentage of articles by non-U.S. based authors dropped from 60% to 41% at *REStud* and from 34% to 28% at *Econometrica*. There was little change at the *AER*, *JPE* and *QJE*.

<sup>76</sup>The source of the early 1970's data is not clearly labeled and there is some chance that the 1970-1972 peak is due to resubmissions being grouped in with new submissions in those years.

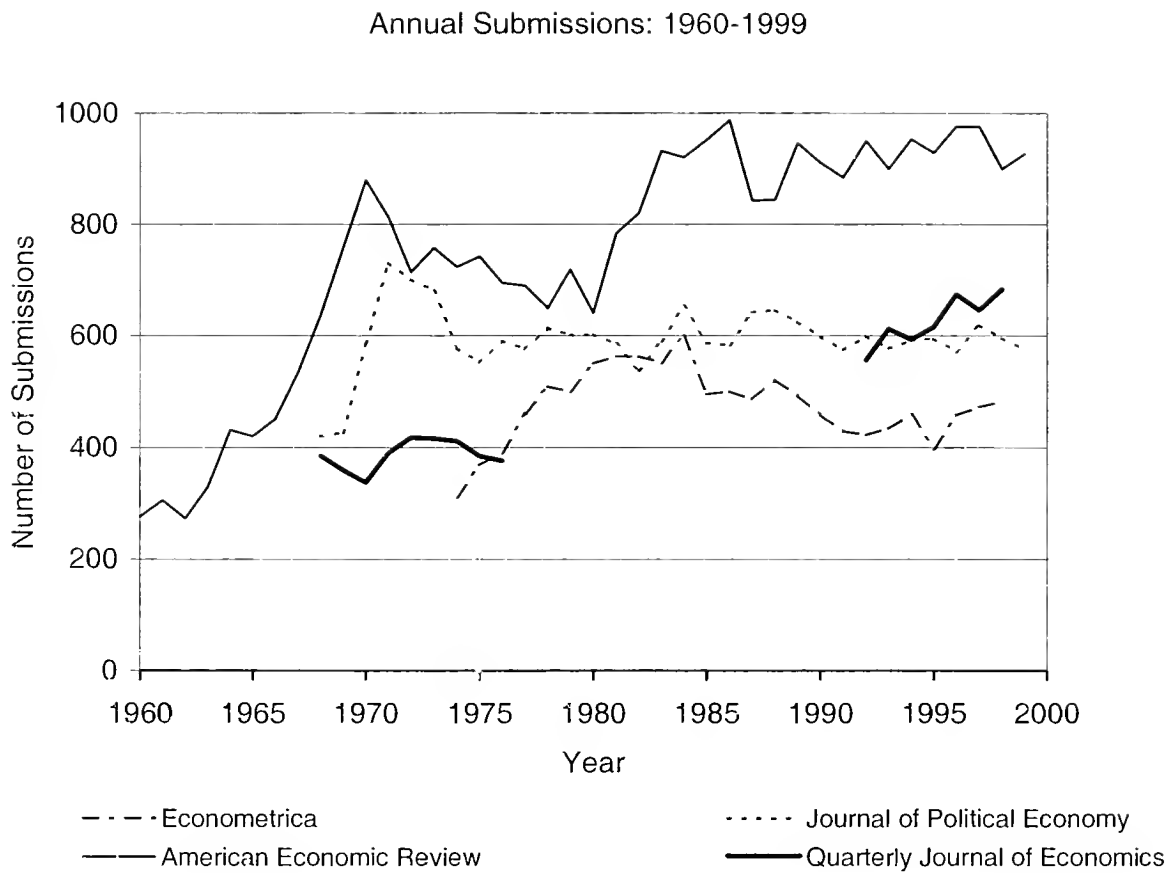


Figure 5: Submissions to top journals

The figure graphs the number of new papers submitted to the *AER*, *Econometrica*, *JPE* and *QJE* in various years since 1960.

that for all their problems, the simple measures above ought to have some power to pick up a large growth in the profession. They also suggest that the common impression that the profession is much larger now than in 1970 may reflect a mistaken recollection of when the earlier growth occurred.

### 6.3 Growth and submit-accept times

In this section I will discuss in turn each of the three arguments mentioned for why the growth of the profession might lead to a slowdown of the publication process.

#### 6.3.1 Editor workloads

First, I noted that editors who are busier may be less likely to give clear instructions about what they'd like to see in a revision and may more often ask referees to review revisions. I see this potential explanation as hard to support, because it is hard to find the exogenous increase in workloads on which it is based.

Editors' workloads have two main components: spending a small amount of time on a large number of submissions that are rejected and spending a large amount of time on the small number of papers that are accepted. To obtain a measure of the first component one would want to adjust submission figures to reflect changes in the difficulty of reading papers and in the number of editors at a journal. Overall submissions have not increased much. Articles are 50 to 100 percent longer now than in 1970. The fraction of submissions that are notes or comments must also have declined. At the same time, however, there have been substantial increases in the number of editors who divide the workload at most journals: the *AER* went from one editor to four in 1984; *Econometrica* went from 3 to 4 in 1975 and from 4 to 5 in 1998; the *JPE* went from 2 to 4 in the mid 1970's and from 4 to 5 in 1999; *REStud* went from 2 to 3 in 1994. Hence, I wouldn't think that the rejection part of editors' workloads should have increased much. The other component should have been reduced because journals are not publishing more papers (see Table 12) and there are more editors dividing the work. I could believe that this part of an editor's job has not become less time-consuming because editors are trying to guide more extensive revisions, but would regard this as switching from increased workloads to changes in norms as the basis for the explanation.

### 6.3.2 Familiarity with submissions

To examine the idea that in the old days editors were able to review papers more quickly because they were more likely to have seen papers before they were submitted I included in the 1990's submit-accept times regression of Table 7 a dummy variable *JournalHQ* indicating whether any of a paper's authors were affiliated with the journal's home institution.<sup>77</sup> The regression yields no evidence that editors are able to handle papers they have seen before more quickly.<sup>78</sup> There could be confounding effects in either direction, e.g. editors may feel pressure to subject colleagues' papers to a full review process, they may ask colleagues to make fewer changes than they would ask of others, they may give colleagues extra chances to revise papers that would otherwise be rejected, etc., but I feel the lack of an effect is still fairly good evidence that the review process is not greatly affected by whether editors have seen papers in advance.<sup>79</sup>

### 6.3.3 Competition

The final potential explanation mentioned above is that the review process may have lengthened because journals have raised quality standards in response to the increased competition among authors for space in the journals. This explanation is naturally addressed by asking whether competition has increased and whether increases in competition would lead to longer review times.

As mentioned above, the evidence from society membership and journal submissions suggests that the relevant population of economists has increased only moderately.<sup>80</sup>

A second relevant factor is how the number of articles published has changed. Articles are now longer than they used to be. While some journals have responded to this by increasing their annual page totals, others have tended to keep their page totals fixed and reduced the number of articles they print. Table 12 illustrates this by reporting the average

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<sup>77</sup>In constructing this variable I regarded the *QJE* as having both Harvard and MIT as home institutions and the *JPE* as having Chicago as its home. Other journals were treated as having no home institution, because it is my impression that editors at the other journals generally do not handle papers written by their colleagues.

<sup>78</sup>Laband et al (1990) report that papers by Harvard authors had shorter submit-accept times at *REStat* in 1976 - 1980.

<sup>79</sup>Laband and Piette (1994a) report that papers by authors who share a school connection with a journal are more widely cited and interpret this as evidence that editors are not discriminating in favor of their friends. Their school connection variable is much looser than those I've considered would include, for example, any instance in which one author of a paper went to the same graduate school as any associate editor of the journal.

<sup>80</sup>An increased emphasis on journal publications rather than books might, however, mean that the number of economists trying to write articles has grown more.

number of full length articles published in each journal in each decade.<sup>81</sup> At *Econometrica* and the *JPE* there has been a substantial decline in the number of articles published. Comments and notes, which once constituted about one quarter of all publications, have also almost disappeared at the *JPE*, *QJE* and *REStud*. As a result, one would expect that a higher proportion of the submissions to these journals are also competing for the available slots for articles.

Table 12: Number of full length articles per year in top journals

Journal	Number of articles per year		
	1970 - 1979	1980 - 1989	1990 - 1997
<i>American Economic Review</i>	53	50	55
<i>Econometrica</i>	74	69	46
<i>Journal of Political Economy</i>	71	58	48
<i>Quarterly Journal of Economics</i>	30	41	43
<i>Review of Economics Studies</i>	42	47	39

The table lists the average number of articles in various journals in different years. The counts reflect an attempt to distinguish articles from notes, comments and other briefer contributions.

A third relevant factor is how the incentives for authors to publish in the top journals has changed. Since 1970 a tremendous number of new economics journals has appeared. This includes the top field journals in most fields. One might imagine that the increase in competition on the journal side might have forced top journals to lower their acceptance threshold and may also have reduced the gap between authors' payoffs from publishing in the top journals and their payoffs from publishing in the next best journals. Surprisingly, the opposite appears to be true.

To explore changes in the relative status of journals, I used data from ISI's *Journal Citation Reports* and from Laband and Piette (1994b) to compute the frequency with which recent articles in each of the journals listed in Table 1 were cited in 1970, 1980, 1990 and 1998. Specifically, for 1980, 1990 and 1998 I calculated the impact of a typical article

<sup>81</sup>There is no natural consistent way to define a full length article. In earlier decades it was common for notes as short as three pages and comments to be interspersed with longer articles rather than being grouped together at the end of an issue. Also, some of the papers that are now published in separate sections of shorter papers are indistinguishable from articles. For the calculation reported in the table most papers in *Econometrica* and *REStud* were classified by hand according to how they were labeled by the journals and most papers in the other journals were classified using rules of thumb based on minimum page lengths (which I varied slightly over time to reflect that comments and other short material have also increased in length).

in journal  $i$  in year  $t$  by

$$CiteRatio_{it} = \frac{\sum_{y=t-9}^t c(i, y, t)}{\hat{n}(i, t-9, t)},$$

where  $c(i, y, t)$  is the number of times papers that appeared in journal  $i$  in year  $y$  were cited in year  $t$  and  $\hat{n}(i, t-9, t)$  is an estimate of the total number of papers published in journal  $i$  between year  $t-9$  and year  $t$ .<sup>82</sup> The data that Laband and Piette (1994a) used to calculate the 1970 measures are similar, but include only citations to papers published in 1965-1969 (rather than 1961-1970).<sup>83</sup> Total citations have increased sharply over time as the number of journals has increased and the typical article lists more references. To compare the relative impact of top journals and other journals at different points in time, I define a normalized variable,  $NCiteRatio_{it}$ , by dividing  $CiteRatio_{it}$  by the average of this variable across the “top 5” general interest journals, i.e. *AER*, *Econometrica*, *JPE*, *QJE* and *REStud*.<sup>84</sup>

Table 13 reports the mean value of  $NCiteRatio$  for four groups of journals in 1970, 1980, 1990 and 1998. The first row reports the mean for next-to-top general interest journals for which I collected data: *Economic Journal*, *International Economic Review*, and *Review of Economics and Statistics*. The second row gives the mean for eight field journals, each of which is (at least arguably) the most highly regarded in a major field.<sup>85</sup> The third row gives the mean for the other economics journals for which I collected data.<sup>86</sup> The table clearly indicates that there has been a dramatic decline in the rate at which articles in the second tier general interest journals and the top field journals are cited relative to the rate at which articles at the top general interest journals are cited. While one could worry that some of the effect is due to my classification reflecting my current understanding of the relative status of journals, the contrast between 1980 and 1998 is striking in the raw data (see Table 20 of Appendix C.) In 1980 a number of the field journals, e.g. *Bell*, *JET*, *JLE*,

<sup>82</sup>The citation data include all citations to shorter papers, comments, etc. The denominator is computed by counting the number of papers that appeared in the journal in years  $t-2$  and  $t-1$  (again including shorter papers, etc.) and multiplying the average by ten. When a journal was less than ten years old in year  $t$  the numerator was inflated assuming that the journal would have received additional citations to papers from the prepublication years (with the ratio of citations of early to late papers matching that of the *AER*.)

<sup>83</sup>In a few cases where Laband and Piette did not report 1970 citation data I substituted an alternate measure reflecting how often papers published in 1968-1970 were being cited in 1977 (relative to similar citations at the top general interest journals.)

<sup>84</sup>The Laband and Piette (1994a) data only give relative citations and thus I can not compare absolute citation numbers in 1970 and later years.

<sup>85</sup>They are *Journal of Development Economics*, *Journal of Econometrics*, *Journal of Economic Theory*, *Journal of International Economics*, *Journal of Law and Economics*, *Journal of Public Economics*, *Journal of Urban Economics*, and the *RAND Journal of Economics* (formerly the *Bell Journal of Economics*).

<sup>86</sup>This includes two general interest journals (*Canadian Journal of Economics* and *Economic Inquiry*) and five field journals.

*JMonetE*, were about as widely cited as the top general interest journals. In 1998, the most cited field journal has only half as many cites as the top journals. Looking at the “other general interest journals” listed in Table 20 it is striking that the even the fourth highest in a 1980 ranking (*Economic Inquiry* at 0.44) has an *NCiteRatio* well above the top journal in the 1998 rankings (the *EJ* at 0.33).

Table 13: Changes in journal status: citations to recent articles relative to citations to top five journals

Set of journals	Mean of <i>NCiteRatio</i> for journals in group			
	1970	1980	1990	1998
Next to top general interest	0.71	0.65	0.37	0.28
Top field journals	0.75	0.69	0.52	0.30
Other journals	0.30	0.39	0.25	0.15

The table illustrates the relative frequency with which recent articles various groups of journals have been cited in different years. (Citations to the top five journals are normalized to one.) The variable *NCiteRatio* and the sets of journals are described in the text. The raw data from which the means were computed are presented in Table 20 of Appendix C.

The data above can not tell us whether the quality of papers in the top journals has improved or whether instead the average quality of papers in the top field journals has declined as more journals divide the pool of available papers. They also can not tell us whether the top journals are now able to attract much more attention to the papers they publish (in which case authors would have a strong incentive to compete for scarce slots) or whether there is no journal-specific effect on citations and papers in the top journals are just more widely cited because they are better (in which case authors receive no extra benefit and would have no increased desire to publish in the top journals). Combining the citation data with the slight growth in the profession and the slight decline in the number of articles top journals publish, however, my inference would be that there is now substantially more competition for space in the top journals.

The second empirical question that thus becomes important is whether (and by how much) an increase in the status of a journal leads to a lengthening of its review process. I noted when presenting my very first table of submit-accept times (Table 1), that the review process is clearly most drawn out at the top journals. In a cross-section regression of the mean submit-accept time of a journal in 1999 on its citation ratios (for 22 journals)



I estimate the relationship to be (with t-statistics in parentheses)

$$\begin{aligned} \text{MeanLag}_{i99} &= 14.6 + 5.8\text{NCiteRatio}_{i98}. \\ (8.7) \quad (1.8) \end{aligned}$$

The coefficient of 5.8 on *NCiteRatio* indicates that as a group the top general interest journals have review processes that are about 5.8 months longer than those at almost never cited journals. The *QJE* is an outlier in this regression. If it is dropped the coefficient on *NCiteRatio* increases to 11.1 and its t-statistic increases to 3.3.

The data on submit-accept times and citations for various journals also allow me to examine how submit-accept times for each journal have changed over time as the journal moves up or down in the journal hierarchy. Table 14 presents estimates of the regression

$$\begin{aligned} \text{MeanLag}_{it} - \text{MeanLag}_{it-\Delta t} &= \alpha_0 \frac{\text{NCiteRatio}_{it} - \text{NCiteRatio}_{it-\Delta t}}{\text{NCiteRatio}_{it-\Delta t}} + \alpha_1 \text{Dum7080}_{t\Delta t} \\ &+ \alpha_2 \text{Dum8090}_{t\Delta t} + \alpha_3 \text{Dum9098}_{t\Delta t} + \epsilon_{it}, \end{aligned}$$

where  $i$  indexes journals and the changes at each journal over each decade are treated as independent observations.<sup>87</sup> In the full sample, I find no relationship between changes in review times and changes in journal citations. The 1990-1998 observation for the *QJE* is a large outlier in this regression. One could also worry that it is contaminated by an endogeneity bias — one reason why the *QJE* may have moved to the top of the citation ranking is that its fast turnaround times may have allowed it to attract better papers.<sup>88</sup> When I reestimate the difference specification dropping this observation (in the second column of the table), the coefficient estimate on the fraction change in the normalized citation ratio increases to 5.3 and the estimate becomes significant.<sup>89</sup> The data on within-journal differences can thus also support a link between increases in a journal's status and its review process lengthening. Hence, for the second time in this paper (the other being page lengths) I have identified both a change in the profession and a link between this change and slowing review times.

How much of the slowdown over the last 30 years can be attributed to increases in competition for space in the top journals? The answer depends both on which regression estimate one uses and on what one assumes about overall quality/status changes. On the

<sup>87</sup>Where the 1990 data are missing I use the 1980 to 1998 change as an observation.

<sup>88</sup>In part because the data are not well known I do not think it is likely that the reverse relationship is generally very important.

<sup>89</sup>The high  $R^2$ 's in both regressions reflect that the contributions of the dummies being included in the  $R^2$ . If these are not counted the  $R^2$  of the second regression is 0.15.

Table 14: Effect of journal prestige on submit-accept times

Independent Variables	Dep. var.: $\Delta MeanLag_{it}$	
	Full	Sample: No QJE 98
$\frac{\Delta NCiteRatio_{it}}{NCiteRatio_{it-\Delta t}}$	0.8 (0.4)	5.3 (2.4)
$Dum7080_{it}$	5.7 (3.1)	6.6 (4.0)
$Dum8090_{it}$	5.5 (5.0)	6.4 (6.4)
$Dum9098_{it}$	2.1 (1.9)	4.1 (3.7)
Number of Obs.	44	43
$R^2$	0.55	0.65

The table reports regressions of changes in mean submit-accept times (usually over a ten year interval) on the fraction change in  $NCiteRatio$  over the same time period. The data include observations on 23 journals for a subset of 1970, 1980, 1990 and 1999 (or nearby years). T-statistics are in parentheses.

low side, if one believes that most of the change in relative citations is just an accurate reflection of the dilution of the set of papers available to the next tier journals, or if one uses the estimate from the full sample difference regression, the answer would be about none. On the high side, one might argue that there is just as much competition to publish in, say, *REStat* today as there was in 1970. *REStat* has slowed down by about 10 months since 1970, while the slowdown at the non-*QJE* top journals averages 14 months. This comparison would indicate that four months of the slowdown in the non-*QJE* top journals is due to the higher standards that the top journals now impose. This answer is also what one gets if one uses the coefficient estimate from the difference regression that omits the 1990-1998 change in the *QJE* and assumes that *REStat's* status has been constant. My view would be that it is probably easier to publish in *REStat* (or *JET*) now than it once was and that increases in competition probably account for two or three months of the slowdown at the top journals.

## 7 Changes in social norms

I use the term social norm to refer to the idea that the structure of the publication process is determined by editors' and referees' understandings of what is "supposed" to be done with

submissions. Social norms may reflect economists' preferences about procedures and/or what they like to see in published papers, but, as in the case of fashions, they may also have little connection with any fundamental preferences. In the publication case, it seems perfectly plausible to me to imagine that in a parallel universe another community of economists with identical preferences could have adopted the norm of just publishing papers in the form in which they are submitted, figuring that any defects in workmanship will reflect on the author.

The general idea that otherwise inexplicable changes in the review process are due to a shift in social norms is inherently unfalsifiable. It is also indistinguishable from the hypothesis that shifts in unobserved variables have caused the slowdown. One can, however, examine whether particular explanations for why social norms might change are supported by the data. In this section I examine the explanation proposed in Ellison (2000) for why social norms might tend to shift over time in the direction of placing an increasing emphasis on revisions.

### 7.1 The potential explanation

The challenge in constructing a model of the evolution of norms is to envision an environment in which it is plausible that norms would slowly but continually evolve over the course of decades. On the most abstract level, the idea behind the model of Ellison (2000) is that such a dynamic is natural in a perturbation of a model with a continuum of equilibria. In the case of journals, a continuum of equilibria can result when an arbitrary convention for weighting multiple dimensions of quality must be adopted. The more specific argument for why social norms may come to place more emphasis on revisions is that a shift may be driven by economists' struggles to understand why their papers are being evaluated so harshly by the same "top" journals that regularly publish a large number of lousy papers.

The mechanics of the model are that papers are assumed to vary in two quality dimensions,  $q$  and  $r$ . I generally think of  $q$  as reflecting the clarity and importance of the main contribution of a paper and  $r$  as reflecting other quality dimensions, e.g. completeness, exposition, extensions, etc., that are more often the focus of revisions. Alternately, one can also think of  $q$  as reflecting the author's contributions and  $r$  as reflecting the referees'. The timing of the model is that in each period authors first allocate some fraction of their time to developing a paper's  $q$ -quality, referees then assess  $q$  and report the level of  $r$  the paper would have to achieve to be publishable, authors then devote additional time to improving the  $r$  of their paper, and finally the editor fills the journal by accepting the papers that are

best in the prevailing social norm. Under the social norm,  $(\alpha, z)$ , papers are regarded as acceptable if and only if  $\alpha q + (1 - \alpha)r \geq z$ .

Because the acceptance set has a downward-sloping frontier in  $(q, r)$  space, authors of papers that turn out to have a very high  $q$  need only spend a little time adding  $r$ -quality to ensure that their papers will be published. Authors of papers with intermediate  $q$ , however, will spend all of their remaining time improving  $r$ , but will still fall short with some probability. At the end of each period, each economist revises his or her understanding of the social norm given observations about the level of  $r$  he or she was told was necessary on his or her own submissions and given observations of the  $(q, r)$  of published papers.

Author/referees will have to reconcile conflicting evidence whenever the community of referees tries to hold authors to an impossibly high standard, i.e. one that would not allow the editor to fill the journal. In this case, authors will feel that the requests referees are making of them are demanding (as they expected), and will be surprised to see a set of papers that fall short of their understanding of the standard being accepted. The distribution of paper qualities that is generated when  $q$  is determined initially and later attempts at marginal improvements focus on  $r$  is such that the unexpectedly accepted papers will have relatively low  $q$ 's and moderate to high  $r$ 's in the distribution of resubmitted papers. Economists rationalize the acceptance of these papers by concluding that overall quality standards must be lower than they had thought *and* that  $r$  must be relatively more important than they had thought. Any force that leads referees to always try to hold authors to a level of overall quality level that is slightly too high to be feasible will lead to a slight continual drift in the direction of emphasizing  $r$ . In Ellison (2000) this is done by assuming that the continuum of correct beliefs equilibria are destabilized by a cognitive bias that makes authors think that their work is slightly better than others perceive it to be.

What evidence might one look for in the data to help evaluate this suggestion for why social norms may tend to drift? First, given that the model views social norms as a somewhat arbitrary standard evolving within a community of author/referees, one might expect in such a model to see social norms evolving differently in different isolated groups. Second, what the model predicts is that norms should evolve slowly from whatever standard the population believes to hold at a point in time. As a result, the model predicts that review times will display hysteresis. For example, a transitory shock like the temporary appointment of an editor who has a personal preference for requiring extensive revisions would have a permanent impact on standards even after the editor has been replaced. Finally, the model views the slowdown as a shift over time in the acceptance frontier in

$(q, r)$ -space. One would thus want to see both that there is a downward sloping acceptance frontier and that the slope of the frontier has shifted over time to place more emphasis on  $r$ .

## 7.2 Evidence

In this subsection I will discuss some evidence relevant to the first and third predictions mentioned above.

### 7.2.1 Are norms field-specific?

Social norms develop within an interacting community. Because economists typically only referee papers in their field and receive referee reports written by others in their field, the evolutionary view suggests that somewhat different norms may develop in different fields. (Differences will be limited by economists' attempts to learn about norms from their colleagues and by the prevalence of economists working in multiple fields.) Trivedi (1993) notes that econometrics papers published in *Econometrica* between 1986 and 1990 had longer review times than other papers. Table 15 provides a much broader cross-field comparison. It lists the mean submit-accept times for papers in various fields published in top five journals in the 1990's. The data indicate that economists in different fields have very different experiences with the publication process (and these differences are jointly highly significant). There is, however, limited overlap in what is published across journals, and in our standard regression with journal fixed effects and journal specific-trends, the differences across fields are not jointly significant.<sup>90</sup> It is thus hard to say from just the data on general interest journals whether different fields have developed different norms or if it is just that different journals have different practices.

Comparing Table 15 with Table 1 it is striking that the fields with the longest review times at general interest journals seem also to have long review times at their field journals. For example, the slowest field journal listed in Table 1 is the *Journal of Econometrics*. There are eleven fields listed in Table 15 for which I also have data on a top field journal. For these fields, the review times in the two tables is 0.81. I take this as clear evidence that there are field-specific differences in author's experiences. This data can not, however, tell us whether the differences in review times are due to inherent differences in the complexity, etc. of papers in the fields or whether they just reflect arbitrary norms that have developed

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<sup>90</sup>Hence, Trivedi's finding does not carry over to this larger set of fields and journals. The p-value for a joint test of equality is 0.12.

Table 15: Total review time by field in the 1990's

Field	# of papers	Mean s-a time	Field	# of papers	Mean s-a time
Econometrics	148	25.7	Macroeconomics	282	20.4
Development	24	24.7	International	69	19.3
Industrial org.	108	23.2	Political econ.	30	18.7
Theory	356	22.9	Public finance	60	17.9
Experimental	35	22.5	Productivity	15	16.2
Finance	117	21.6	Environmental	10	15.5
Labor	105	20.8	Law and econ.	13	14.5
History	12	20.6	Urban	13	14.4

The table lists the mean submit-accept time (or submit-final resubmit for *Econometrica*) in months for papers in each of sixteen fields published in top five journals in the 1990's, along with the number of papers in the field for which the data were available.

differently in different fields.

One way in which I thought field-specific norms might be separated from journal and complexity effects was by looking at finer field breakdowns. Table 16 provides a similar look at mean submit-accept times for the ten subfields into which I divided microeconomic theory. My hope was that such breakdowns could make field-specific differences in complexity less of a worry, increase the number of fields that could be compared within each journal, and lessen the the problem of *JPE* theory papers being inappropriately compared with very different *Econometrica* theory papers. The differences between theory subfields indeed turn out to be very large, and they are also statistically significant at the one percent level in regression like our standard regression but with more field dummies.<sup>91</sup> I take this as suggestive that there are field-specific publishing norms within microeconomic theory.

### 7.2.2 Tradeoffs between $q$ and $r$

As described above, Ellison (2000) suggests that the slowdown of the economics review process can be thought of as part of a broader shift in the weights that are attached to different aspects of paper quality. The models' framework is built around an assumption that referees and editors make tradeoffs between different aspects of quality — papers with more important main ideas (high  $q$ -quality) will be held to a lower standard on dimensions of exposition, completeness, etc. ( $r$ -quality). It predicts that over time norms will increasingly

<sup>91</sup>In fact, the full set of thirty-one dummies for the fields listed in Table 17 is jointly significant at the one percent level.

Table 16: Total review time for theory subfields in the 1990's

Field	# of papers	Mean s-a time	Field	# of papers	Mean s-a time
General equil.	34	27.9	Learning	13	21.6
Game theory	82	26.3	Contract theory	59	21.2
Unclassified	32	22.3	Auction theory	13	19.3
Decision theory	28	22.1	Social choice	19	19.0
Price theory	59	21.9	Welfare economics	17	16.9

The table lists the mean submit-accept time (or submit-final resubmit for *Econometrica*) for papers in each of ten subfields of microeconomic theory published in top five journals in the 1990's, along with the number of papers in the field for which the data were available.

emphasize  $r$ -quality.

To assess the assumption and the conclusion we would want to look for two things: evidence that journals do make a  $q$ - $r$  tradeoff and evidence that the way in which the  $q$ - $r$  tradeoff is made has shifted over time. The idea of this section is that review times may be indicative of how much effort on  $r$ -quality is required of authors and that two available variables that may proxy for  $q$ -quality are whether a paper is near the front or back of a journal issue and how often it has been cited.  $Q$ - $r$  tradeoffs can then be examined by including two additional variables in the submit-accept time regression. *Order* is the order in which an article appears in its issue in the journal, e.g. one indicates that a paper was the lead article, two the second article, etc.  $\text{Log}(1 + \text{Cites})$  is the natural logarithm of one plus the total number of times the article has been cited.<sup>92</sup> Summary statistics for these variables can be found in Table 6. Note that a consequence of the growth in the number of economics journals is that the mean and standard deviation of  $\text{Log}(1 + \text{Cites})$  are not much lower for papers published in the 1990's than they are for papers published in the earlier decades.

The regression results provide fairly strong support for the idea that journals make a  $q$ - $r$  tradeoff. In all three decades papers that are earlier in a journal issue spent less time in the review process. In all three decades papers that have gone on to be more widely cited spent less time in the review process.<sup>93</sup> Several of the estimates are highly significant.

The regressions does not, however, provide evidence to support the idea that there has

<sup>92</sup>The citation data were obtained from the online version of the *Social Science Citation Index* in late February 2000.

<sup>93</sup>Laband et al (1990) had found very weak evidence of a negative relationship between citations and the length of the review process in their study of papers published in *REStat* between 1976 and 1980.

been a shift over time to increasingly emphasize  $r$ . Comparisons of the regression coefficients across decades can be problematic because the quality of the variables as proxies for  $q$  may be changing.<sup>94</sup> The general pattern, however, is the coefficients on *Order* and  $\text{Log}(1+\text{Cites})$  are getting larger over time. (The increase is not so sharp if one thinks of the magnitudes of the effects relative to the mean review time.) This is not what would be expected if  $q$ -quality were becoming less important.

## 8 Conclusion

Many other academic fields have experienced trends similar to those in economics. The process of publishing has become more drawn out and the published papers are observably different (Ellison 2000). Robert Lucas (1988) has said of economic growth that “Once one begins to appreciate the importance of long-run growth to macroeconomic performance it is hard to think about anything else.” While I would not go so far as to advocate devoting a comparable share of journal space to the study of journal review processes, one could argue from the fact that review processes have changed so much and that they have a large impact not only on the amount of progress that is made by economists studying economic growth but also on the productivity of all other social and natural scientists that they are a much more important topic for research.

In trying to understand why the economics publishing process has become more drawn out, I’ve noted that there are many seemingly plausible ways in which changes in the review process could result from changes in the economics profession. I find some evidence for a few effects. Papers are getting longer and longer papers take longer to review. This may account for one or two months of the slowdown. The top journals appear to have become more prestigious relative to the next tier of journals. Their ability to demand more of authors may account for another three months.

My greatest reaction to the data, however, is that I don’t see that there are many fundamental differences between the economics profession now and the economics profession in 1970. The profession doesn’t appear to be much larger. It doesn’t appear to be much more democratic. I can’t find the increasing specialization that I would have expected if economic research were really much harder and more complex than it was thirty years ago.

I have also found evidence for very few of the potential explanations for why changes in the profession would have slowed review times if such changes had occurred. I am led

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<sup>94</sup>For example, I know that the relationship between the order in which an article appears and how widely cited it becomes has strengthened over time. This suggests that *Order* may now be a better proxy for  $q$ .



to conclude that perhaps there is no reason why economics papers must now be revised so extensively prior to publication. The changes could instead reflect a shift in arbitrary social norms that describe our understanding of what kinds of things journals are *supposed* to ask authors to do and what published papers *should* look like.

I am sure that others will be able to think of alternate equilibrium explanations for the slowdown that merit investigation. One possibility is that there are simply fewer important ideas waiting to be discovered. Another is that increasingly long battles with referees may be due to referees becoming more insecure or spiteful. Another is that economists today may now have worse writing skills, e.g. being worse at focusing on and explaining a paper's main contribution, perhaps due to trends in what is taught in high school and college. Finally, there is another multiple equilibrium story: we may spend so much time revising papers because authors (cognizant of the fact that they will have to revise papers later) strategically send papers to journals before they are ready. I certainly believe that such strategic behavior is widespread, but also believe that my data on the growth of revisions *understates* the increase in polishing efforts. Looking back at published papers from the 1970's I definitely get the impression that even the first drafts of today's papers have been rewritten more times, have more thorough introductions (with much more spin), have more references, consider more extensions, etc.

What future work do I see as important? First, the social norms explanation I fall back on is very incomplete. The crucial question it raises is why social norms have changed. Further work to develop models of the evolution of social norms would be useful.

The empirical approach of this paper to the general question of why standards for publishing have changed follows what is the standard practice in industrial organization these days. To understand a general phenomenon I've focused on one industry (economics) where the phenomenon is observed, where data were available, and where I thought I had or could gain enough industry-specific knowledge to know what factors are important to consider. Economics, however, is just one data point of the many that are available. Many fields have similar (though usually less severe) slowdowns and many others do not. Different disciplines will also differ in many of the dimensions studied here, e.g. in their rates of growth. An inter-disciplinary study would thus have the potential to provide a great deal of insight.

Studies that look in more depth at the changes in economics publishing would also be valuable. For example, I would be very interested to see a descriptive study of how the contents of referees' reports and editors' letters have changed over time. To better

understand the causes of multi-round reviews it would also be very useful to see whether a blind observer (be they an experienced editor, a graduate student or a writing expert) can predict how long papers ended up taking to get accepted from examining the first drafts, and if so what characteristics of papers they use.

The suggestion that the review process at economics journals might not be optimal should not be surprising to economists. While there are lots of implicit incentives and some nominal fees or payments, almost everything about the process is unpriced. Most readers are not paying directly in a way that makes journal prices reflect readers' demand for the articles; authors are not paid for their papers nor can they negotiate with journals and reach agreements with payments going one way or the other in exchange for making or not making revisions or to change publication decisions; referees are not paid anything approaching their time cost and do not negotiate fees commensurate with the quality of their contributions to a particular paper; etc.

The idea that the nature of the journal review process is largely determined by arbitrary social norms can ironically be thought of as an optimistic world view. It suggests that the review process could be changed dramatically if economists simply all decided that papers should be assessed differently. Newspapers and popular magazines publish articles and columns about economics a few days after they are written. Given the tremendous range between this and the current review process in economics (or an even more drawn out process if desired), it would seem valuable to have a discussion in the profession about whether the current system captures economists' joint preferences.

Further research into the effects of the review process (as suggested in the *JPE*'s 1990 editors' report) could enlighten this discussion.<sup>95</sup> A simple project suggested to me by Ilya Segal would be to collect for a random sample of papers the version initially submitted to a journal and the first and second revisions and blindly allocate them to three graduate students. Seeing independent ratings of the drafts could teach us a lot about the value-added of the process.

Finally, although not directly related to the slowdown, the paper suggests other avenues for research into the economics profession. The observations about the profession I find most striking are that economists do not seem to be becoming more specialized and that on a relative citations basis the top journals are becoming more dominant. To see whether power is becoming increasingly concentrated in the hands of the top journals it would be interesting

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<sup>95</sup>The one piece of research I'm aware of on the topic is a Laband's (1990) study of citations for 75 papers published in various journals in the late 1970s. It found that papers for which the ratio of the time authors spent on revisions to the length of the comments they receive was larger were more widely cited.

to update Sauer's (1988) study of the relative value (in salary terms) of publications in various journals and also look for changes in what journals economists must publish in to obtain and keep a position.<sup>96</sup> It may also be interesting for theorists to think about whether the increased status of the top journals may be a natural consequence of the proliferation of journals (or some other trend). With the recent growth (and potential future explosion) of internet-based paper distribution, this may help us predict whether journals will continue to direct the attention of the profession or whether great changes are in store.

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<sup>96</sup>Sauer (1988) found that a publication in the 10th best journal was worth about 60 percent of a publication in the top journal and that a publication in the 80th best journal was worth about 20 percent of a publication in the top journal.

## Appendix A

The idea of the field-to-field distance measure is to regard fields as close together if authors who write in one field also tend to write in the other. In particular, for pairs of fields  $f$  and  $g$  I first define a correlation-like measure by

$$c(f, g) = \frac{P(f, g) - P(f)P(g)}{\sqrt{P(f)(1 - P(f))P(g)(1 - P(g))}},$$

where  $P(f, g)$  is the fraction of pairs of papers by the same author that consist of one paper from field  $f$  and one paper from field  $g$  (counting pairs with both papers in the same field as two such observations), and  $P(f)$  is the fraction of papers in this set of pairs that are in field  $f$ . I then construct a distance measure,  $d(f, g)$ , which is normalized so that  $d(f, f) = 0$  and so that  $d(f, g) = 1$  when writing a paper in field  $f$  neither increases nor decreases the likelihood that an author will write a paper in field  $g$  by

$$d(f, g) = 1 - \frac{c(f, g)}{\sqrt{c(f, f)c(g, g)}}.^{97}$$

I classified papers as belonging to one of thirty one fields (again using JEL codes and other rules). The field breakdown is the same as in the base regression except that I have divided macroeconomics into three parts, international, finance, and econometrics into two parts each, and theory into ten parts. See Table 17 for the complete list. To get as much information as possible about the relationships between fields and about editors with few publications in the top five journals, the distance matrix and editor profiles were computed on a dataset which also included notes, shorter papers, and papers in three other general interest journals for which I collected data: the *AER's Papers and Proceedings* issue, *Brookings Papers on Economic Activity*, and *REStat*. Papers that were obviously comments or replies to comments were dropped. All years from 1969 on were pooled together.

To illustrate the functioning of the distance measure, Table 17 lists for each of the thirty-one fields up to three other fields that are closest to it. I include fewer than three nearby fields when there are fewer than three fields at a distance of less than 0.99.

To illustrate how the editor imputation is working in different areas of economics, I report in Table 18 the *EditorDistance* variable and the identity of the imputed editor for all observations in the regression described in Table 10 belonging to the four economists having the largest number of articles in the 1990's in the top five journals among those working primarily in microeconomic theory, macroeconomics, econometrics and empirical microeconomics: Jean Tirole, Ricardo Caballero, Donald Andrews and Alan Krueger.<sup>98</sup> Editors names are in plain text if the editor's identity was known. Bold text indicates that it was imputed correctly. Italics indicate that it was imputed incorrectly.

<sup>97</sup>The assumption that within-field distances are zero for all fields ignores the possibility that some fields are broader or more specialized than others. I experimented with using measures of specialization based on JEL codes like those in the previous subsection to make the within-field distances different, but cross-field comparisons like this are made difficult by the differences in the fineness and reasonableness of the JEL breakdowns, and I found the resulting measure less appealing than setting all within-field distances to zero.

<sup>98</sup>Note (especially with reference to Krueger) that this is not the same as the economists who contribute the most observations to my regression from these fields given that I lack data on submit-accept times from 1990-1992 at the *JPE* and *AER* and for 1991-1992 at the *QJE*.

Table 17: Closest fields in the field-to-field distance measure

Field	Three closest fields		
Micro theory — unclassified	Industrial org.	Micro - WE	Micro - GE
Micro theory — price theory	Micro - U	Micro - WE	Micro - DT
Micro theory — general eq.	Micro - U	Micro - WE	Micro - GT
Micro theory — welfare econ.	Micro - U	Public Finance	Micro - GE
Micro theory — game theory	Micro - L	Micro - CT	Micro - SC
Micro theory — social choice	Political economy	Experimental	Micro - WE
Micro theory — contract th.	Micro - L	Micro - GT	Micro - U
Micro theory — auctions	Experimental	Micro - CT	Industrial org.
Micro theory — decision th.	Micro - PT	Micro - U	Micro - GT
Micro theory — learning	Micro - GT	Micro - CT	Finance - U
Macro — unclassified	Finance - U	International - IF	Macro - G
Macro — growth	Productivity	Development	Macro - T
Macro — transition	Finance - C	Law and economics	Development
Econometrics — unclassified	Econometrics - TS	—	—
Econometrics — time series	Econometrics - U	—	—
Industrial organization	Micro - U	Micro - CT	Micro - A
Labor	Urban	Public finance	—
International — unclassified	International - IF	Development	Macro - G
International — int'l finance	International - U	Macro - T	Macro - U
Public finance	Micro - WE	Urban	Environmental
Finance — unclassified	Micro - L	Macro - U	Finance - C
Finance — corporate	Micro - U	Macro - T	Micro - CT
Development	Macro - T	International - IF	International - U
Urban	Labor	Law and economics	Public Finance
History	Productivity	Development	Other
Experimental	Micro - A	Micro - SC	Micro - GT
Productivity	Macro - G	Industrial org.	History
Political economy	Micro - SC	Law and economics	Macro - T
Environmental	Public finance	Development	Micro - WE
Law and economics	Political economy	Urban	Macro - T
Other	History	Political economy	Urban

The table reports for each field in the 31-field breakdown the three other fields that are closest to it. The distance measure is derived from an examination of the publication records of authors with at least two publications in seven general interest journals since 1969 as described in the text. A dash indicates that fewer than three fields are at a distance of less than 0.99 from the field in the first column.

Table 18: Examples of editor assignments and distances

Journal & Year	Title	Assumed Editor	<i>Editor Distance</i>
Papers by Jean Tirole			
RES 90	Adverse Selection and Renegotiation in Procurement	<b>Moore</b>	0.56
EMA 90	Moral Hazard and Renegotiation in Agency Contracts	Kreps?	0.71
QJE 94	A Theory of Debt and Equity: Diversity of ...	Shleifer	0.75
EMA 90	The Principal Agent Relationship with an ...	<b>Kreps</b>	0.85
EMA 92	The Principal Agent Relationship with an ...	<i>Kreps</i>	0.85
QJE 97	Financial Intermediation, Loanable Funds, and ...	Blanchard	0.85
RES 96	A Theory of Collective Reputations (with ...	<i>Dewatripont</i>	0.86
JPE 95	A Theory of Income and Dividend Smoothing ...	Scheinkman	0.92
QJE 94	On the Management of Innovation	Shleifer	0.98
JPE 93	Market Liquidity and Performance Monitoring	Scheinkman	1.00
JPE 98	Private and Public Supply of Liquidity	Topel	1.03
JPE 97	Formal and Real Authority in Organizations	Rosen	1.06
Papers by Ricardo Caballero			
QJE 90	Expenditure on Durable Goods: A Case for Slow ...	<i>Blanchard</i>	0.23
QJE 93	Microeconomic Adjustment Hazards and Aggregate ...	<b>Blanchard</b>	0.23
AER 94	The Cleansing Effect of Recessions	<b>Campbell</b>	0.49
EMA 91	Dynamic (S,s) Economies	<i>Deaton</i>	0.68
JPE 93	Durable Goods: An Explanation for their Slow ...	Lucas	0.73
AER 87	Aggregate Employment Dynamics: Building from ...	<b>West</b>	0.79
QJE 96	The Timing and Efficiency of Creative Destruction	Katz	0.97
RES 94	Irreversibility and Aggregate Investment	<b>Dewatripont</b>	1.02
Papers by Alan Krueger			
AER 94	Minimum Wages and Employment: A Case Study ...	<i>Ashenfelter</i>	0.34
QJE 93	How Computers Have Changed the Wage Structure:	<b>Katz</b>	0.37
QJE 95	Economic Growth and the Environment	Blanchard	0.96
AER 94	Estimates of the Economic Returns to Schooling ...	<i>Milgrom</i>	1.04
Papers by Donald Andrews			
EMA 94	The Large Sample Correspondence between Classical ...	<b>Robinson</b>	0.13
EMA 97	A Conditional Kolmogorov Test	<b>Robinson</b>	0.13
EMA 97	A Stopping Rule for the Computation of Generalized ...	<b>Robinson</b>	0.25
EMA 91	Asymptotic Normality of Series Estimators for ...	<b>Hansen</b>	0.59
EMA 94	Optimal Tests When a Nuisance Parameter Is Present ...	<i>Hansen</i>	0.59
EMA 94	Asymptotics for Semiparametric Econometric Models ...	<b>Hansen</b>	0.59
EMA 91	Heteroskedasticity and Autocorrelation Consistent ...	<b>Hansen</b>	0.60
EMA 93	Tests for Parameter Instability and Structural ...	<b>Hansen</b>	0.60
EMA 93	Exactly Median-Unbiased Estimation of First Order ...	<b>Hansen</b>	0.60
RES 95	Nonlinear Econometric Models with Deterministically ...	<i>Jewitt</i>	0.95

The table reports the imputed editor and the values of *EditorDistance* for papers in the 1990's dataset by four authors. The editor's name is in plain text if it was known. It is bold if it was imputed correctly and in italics if it was imputed incorrectly. *EditorDistance* is a measure of how far the paper is from the editor's area of expertise. It is constructed from data on cross-field authoring patterns as described in the text.

## Appendix B

The set of seventeen main fields and the fraction of all articles in the top five journals falling into each category are given in Table 19.

Table 19: Field breakdown of articles in top five journals

Field	Percent of papers		
	1970's	1980's	1990's
Microeconomic theory	26.3	29.5	22.7
Macroeconomics	17.5	15.5	21.3
Econometrics	9.5	9.1	8.7
Industrial organization	8.9	11.0	8.3
Labor	9.8	9.0	8.6
International	6.9	5.4	5.6
Public Finance	6.1	5.3	5.3
Finance	5.2	5.3	7.7
Development	3.8	1.4	1.6
Urban	2.2	0.7	1.1
History	1.1	1.9	1.0
Experimental	0.4	1.3	2.5
Productivity	1.4	1.2	0.9
Political economy	1.1	0.6	1.9
Environmental	0.4	0.4	0.8
Law and economics	0.3	0.3	1.0
Other	3.1	2.3	1.2

The table reports the fraction of articles in the top five journals in each decade that are categorized as belonging to each of the above fields. Data for the 1990's includes data up to the end of 1997 or mid-1998 depending on the journal.

## Appendix C

Table 20: Recent citation ratios: average of top five journals normalized to one

Journal	Value of $NCiteRatio$			
	1970	1980	1990	1998
Top five general interest journals				
<i>American Economic Review</i>	<sup>a</sup> 1.01	1.02	0.73	0.64
<i>Econometrica</i>	0.86	0.95	1.71	1.00
<i>Journal of Political Economy</i>	0.81	1.69	1.11	1.23
<i>Quarterly Journal of Economics</i>	0.94	0.61	0.74	1.37
<i>Review of Economic Studies</i>	1.38	0.74	0.71	0.76
Other general interest journals				
<i>Canadian Journal of Economics</i>	<sup>bc</sup> 0.34	0.24	0.18	0.06
<i>Economic Inquiry</i>	0.26	0.44	0.29	0.15
<i>Economic Journal</i>	0.65	0.78	0.49	0.33
<i>International Economic Review</i>	0.53	0.53	0.26	0.20
<i>Review of Economics and Statistics</i>	0.95	0.65	0.36	0.29
Economics field journals				
<i>Journal of Applied Econometrics</i>			<sup>c</sup> 0.32	0.26
<i>Journal of Comparative Economics</i>		<sup>cd</sup> 0.38	0.24	0.16
<i>Journal of Development Economics</i>		<sup>c</sup> 0.28	0.30	0.16
<i>Journal of Econometrics</i>		<sup>cd</sup> 0.49	0.53	0.36
<i>Journal of Economic Theory</i>	<sup>bc</sup> 0.78	0.69	0.40	0.21
<i>Journal of Environmental Ec. &amp; Man.</i>		<sup>c</sup> 0.46	0.21	0.16
<i>Journal of International Economics</i>		0.35	0.38	0.26
<i>Journal of Law and Economics</i>	0.71	1.26	0.87	0.51
<i>Journal of Mathematical Economics</i>		<sup>cd</sup> 0.42	0.28	0.10
<i>Journal of Monetary Economics</i>		<sup>c</sup> 0.87	0.81	0.45
<i>Journal of Public Economics</i>		<sup>c</sup> 0.56	0.34	0.19
<i>Journal of Urban Economics</i>		<sup>c</sup> 0.61	0.28	0.24
<i>RAND Journal of Economics</i>		1.11	<sup>c</sup> 0.78	0.31
Mean $CiteRatio$ for "Top 5" journals	—	1.46	2.59	3.99

The table reports the measure  $NCiteRatio$  of the relative frequency with which recent articles in each journal were cited in year  $t$ . The last row gives the mean of  $CiteRatio$  for the first five journals listed. Notes: *a* - Value computed as a weighted average of values reported in Laband and Piette for the regular and P&P issues. *b* - Value was not given by Laband and Piette and data instead reflect 1977 citations to 1968-1970 articles. *c* - Journal began publishing during period for which citations were tallied and values are adjusted in accordance with the time-path of citations to the *AER*. *d* - Data are for 1982.



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